IOWA COLLEGE
CHRISTO DUCE
LIBRARY, 1848

THE

AMERICAN NATURALIST

VOL. XXXI. November, 1897. 371

CEPHALIC HOMOLOGIES. A CONTRIBUTION TO THE DETERMINATION OF THE ANCESTRY OF VERTEBRATES.[1]

BY CHARLES SEDGWICK MINOT.

PROFESSOR AT THE HARVARD MEDICAL SCHOOL, BOSTON, MASS.

1. THE AFFINITIES OF AMPHIOXUS.

That Amphioxus has affinities both with Tunicates and with Vertebrates is, with our present knowledge, quite clear; but the relative degree of affinity on the two sides has not been settled. Amphioxus was originally classed with the fishes, but is now universally separated from them. Perhaps the most usual opinion is, that which classes Amphioxus with the Vertebrata, and divides the Vertebrata into two groups:—

I. CEPHALOCHORDA, (*Amphioxus*).

II. VERTEBRATA PROPER, (*Craniota*).

This division recognizes the fact that Amphioxus differs fundamentally from all the true vertebrates, but holds that the concept "Vertebrata" must be greatly extended and essentially modified, so as to admit the lancelet. The first writer known to me to deny that the Branchiostoma could be classed as a vertebrate was Carl Semper in 1875. This position has

[1] Read before the British Association for the Advancement of Science, at Toronto, Canada, August 20, 1897.

hitherto found little favor. Nevertheless, I hold that Amphioxus is more closely related to the Tunicates than to the Vertebrates, and that we ought to establish a group, which might be named Atriozoa, and would comprise Amphioxus and Tunicata.

The systematic value of embryology has been hitherto little recognized, but it certainly is of the greatest significance. It is not true that embryos are alike; on the contrary they show class, ordinal and generic differences from one another. I think, therefore, great stress must be laid upon the very close similarity in the embryonic development of Amphioxus and Tunicates. The fact of this similarity is now so familiar that it is superfluous to dwell upon it. On the other hand, it is desirable to renew attention to the large factor, which interpretation becomes, as soon as we attempt to establish a similarity between the ontogeny of Amphioxus and of the Vertebrates.

As regards the adult organization. The entire absence of lateral eyes and of lateral ears sharply separates both Tunicates and Amphioxus from the Vertebrata, while in the sense organs, they do possess, namely, the pigment spot or so-called eye, and the so-called olfactory pit, they are closely similar to one another, and quite unlike vertebrates. The attempt to identify the eye of Amphioxus with the pineal eye rests upon sheer speculation, and assumes that an organ developed in front of the neuropore is identical with one developed behind it. As regards the "olfactory pit," Kupffer's attempt to demonstrate a lobus olfactorius impar in Vertebrates is obviously unsuccessful. The atrium is another feature common to the Atriozoa, and is so highly distinctive, that it affords, what seems to me, an appropriate name for the whole group. The differences in the development of the atrium in the two classes of the Atriozoa are not, it is generally believed, such as to upset the homology, which is commonly maintained. The pharynx with its endostyle and ciliated bands, shows almost identity in Tunicates and Amphioxus, but only remoter resemblance to the Vertebrate pharynx, even to that of Ammocoetes with its hypobranchial groove. The gill clefts of

Amphioxus, both by their large number and by their peculiar subdivision, by secondary bars, resemble strikingly Tunicate gill clefts, but differ strikingly from those in any of the true vertebrates. How far the sexual organs of the cephalochorda and urochorda are comparable is not yet clear, but they show this striking resemblance that, in both, the sexual elements are discharged by dehiscence and pass through the ectoderm into the atrium, whereas in all vertebrates the sexual elements are discharged into the body cavity, or a duct derived from the body cavity. The excretory organs of Amphioxus in their association with the gills, and in their exterior or ectodermal openings show features which cannot be considered vertebrate, but I venture to express the expectation that homologous organs will be found in Tunicates. Finally as regards the central nervous system, the resemblance, which is almost identity, between Tunicates and Amphioxus has been often emphasized, but if we follow Howard Ayers in his attempt to homologize the parts of the brain in Amphioxus with those in true Vertebrates, we shall become only more and more impressed with the wide divergence between the two.

There remains the muscular system, which presents a striking and genuine resemblance to the metameric musculature of fishes, despite the secondarily acquired asymmetry in Amphioxus. It is a singular coincidence that some of those, who consider the segmentation of the body morphologically so insignificant, that it has no value to prove the kinships of Annelins and Amphioxus, yet consider it of great importance as evidence of the relationship of Amphioxus to the Vertebrates. But if we consider Amphioxus as a type intermediate between Annelids and Tunicates, the segmented musculature has been lost in the latter, an hypothesis which seems to me plausible, for as I have pointed out in my essay on "Senescence and Rejuvenation," all evolution of animals depends not only on the acquisition of characteristics, but also very largely on the loss of characteristics; this loss, as exemplified in the gill clefts and arches of the higher Vertebrates, affects the early embryonic stages, apparently to allow the embryonic material to undergo a new development. The assumption that the Tunicates

have lost segments offers fewer difficulties than the assumption that they acquired segments to develop into the vertebrate type.

If morphological evidence has any value whatever, the conclusion seems to me inevitable that the Tunicates and Amphioxus constitute a natural group, Atriozoa, which are somewhat distantly allied to the Vertebrata.

2. THE CHORDATA.

Zoologists are generally agreed that the Chordata is a natural group, in the sense that the Tunicates, Amphioxus and Vertebrates are more closely allied to one another than with any other known animals. I can find no reason for dissent from this view, it therefore suffices to follow those who have insisted that the problem of the origin of Vertebrates is part of the problem of the origin of Tunicates.

3. THE PRINCIPAL THEORIES OF THE ORIGIN OF VERTEBRATES.

It is, in my judgment, unnecessary to discuss the theories of Adam Sedgwick as to the origin of metameric segmentation,[2] or of Hubrecht that the Nemertines are the ancestral type from which Vertebrates have sprung. The latter I have discussed previously. Of the former I will only remark in passing that it seems strange that Sedgwick has persistently denied the concrescence theory of the embryo, and yet puts it forward as a new idea upon which he founds his hypothesis of metamerism.

There are five theories known to me which certainly require consideration, namely, Gaskell's, Patten's, Bateson's, and the Appendicularia and Annelid theories.

A. *Gaskell's theory* was ably presented in Dr. Gaskell's address, delivered last year at Liverpool before the Sections of Physiology and Zoology. The main points in this theory are that the Vertebrata arose from Crustacea like animals, the intestine of the latter becoming the tubular nervous system of

[2] Of course, not because the theory does not need consideration, but because it deals not directly with the origin of Vertebrates, but with the origin of segmented animals.

the former, while a pharynx arose by the concresence of branchiate crustacean legs, the spaces between the legs becoming gill clefts. There are two fatal objections to this theory. I consider it a first and sufficient objection that it sets aside the evidence that the primary germ layers, ectoderm and entoderm are homologous throughout the metazoa. The evidence on this point is overwhelming. Gaskell's theory leaves a heart between the pharynx and the nervous system, so that to save his theory he has to make two supplementary hypotheses, first, that the crustacean heart disappears, and second, that a new or vertebrate heart is formed by the tips of the legs where they grow together. It is, indeed, difficult to give morphological credence to these two hypotheses. If Gaskell's hypotheses were true, we should expect to see some trace in the ontogeny of Vertebrates of the development of the pharynx by the concrescence of solid outgrowths, but, as well known, the pharynx is a series of hollow entodermal evaginations. As regards the neuron, Gaskell supposes that the ependyma represents the epithelium of the crustacean intestine, while the nervous matter represents that which collects in the walls of the crustacean intestine. This view, which is essential to his hypothesis, overlooks the fact that in Vertebrates the same epithelium produces both ependyma and nerve cells. Very numerous other objections can be raised, and consequently I am ready to go so far as to maintain that the acceptance of Gaskell's theory is impossible.

B. *Patten's theory* seeks in the Arachnid type the ancestral form of Vertebrates. Under Arachnids Patten includes the Arachnida *sensu strictu*, and the Trilobites and Limulus also. It is noteworthy that the original articles by Patten and Gaskell appeared in the same number of the *Quarterly Journal of Microscopical Science*, (August, 1890). I have often wondered whether the juxtaposition was due to editorial design. Patten rests his case chiefly upon comparison of the nervous system, and makes only somewhat uncertain and tentative suggestions, as to the three serious difficulties offered by vertebrate characteristics, namely, 1, the disappearance of the invertebrate and origin of the vertebrate mouth; 2, the origin of the

pharynx; 3, the origin of the notochord. Under these circumstances it is inevitable that the whole hypothesis seems highly tentative. Patten emphasizes the fact that the brain and oesophageal commissures of Arachnids form an angle with the ventral nerve chain, and he compares this with the head bend of Vertebrates, and seeks to homologize the supra-oesophageal and suboesophageal nerves with these of Vertebrates. After a careful study it seems to me that he fails to render these comparisons morphologically justifiable. This failure does away with the only important argument offered by Patten.

C. *Bateson's theory* that Balanoglossus represents an ancestral type of the Chordata has attracted considerable attention, and Wiley apparently considers the theory proven. Bateson seeks to demonstrate a close homology between Balanoglossus and Amphioxus. Yet according to his own observations the structure of Balanoglossus is extremely different in every organ and in every part of every organ from what is found in Amphioxus, except as regards the pharynx, where there occur striking resemblances in the disposition of the gill bars. But Spengel has demonstrated that these are resemblances only, and that the arrangement of the bars in Balanoglossus is so fundamentally different from that in Amphioxus that it is out of the question to deduce one from the other. Again, according to Bateson's observations on the development of Balanoglossus, Tornaria is one of the forms least like the embryonic lancelet, for it entirely lacks the segmented mesoderm, the notochord and elongated medullary plate. Bateson, in my opinion, has drawn exactly the reverse of the conclusions as to the phylogenetic value of Balanoglossus, which are warranted by his own published observations. Spengel's criticisms of Bateson's theory are so conclusive, that it is superfluous to go over the ground again. But two points may be briefly mentioned, if only to escape the charge of trifling with the theory. Bateson lays much emphasis upon a structure, which he names the notochord of Balanoglossus; this so-called "notochord" is a short diverticulum of the cephalad end of the alimentary canal, running into the proboscis; this diver-

ticulum remains hollow throughout life; whether it is of entodermal origin or not is unknown; Spengel considers it probably ectodermal. A notochord is a band of entodermal cells differentiated along the dorsal median line and ending originally at the blastopore, with the walls of which it is fused, and it lengthens by additions at its blastoporic end; it is never hollow and never a canal. These characteristics of a notochord are invariable, but not a single one of them pertains to the alleged notochord of Balanoglossus. The question arises, if two organs are found to have no resemblance to one another, is their homology demonstrated? Surely, a morphologist can give but one answer. My second point touches upon Bateson's views concerning metamerism. These views are based on the tacit assumption that serial repetition of organs or parts, even if irregular, is the same as segmentation of the body. But embryology had demonstrated, before Bateson's time, the true morphological basis of metamerism in segmented animals. This basis is the division of the mesothelium, as above stated, into paired symmetrical blocks. That this is the case is not an opinion, it is merely the summary statement of thousands upon thousands of direct observations. Serial repetitions, therefore, of ectodermal or entodermal organs, without these mesothelial segments, are morphologically not segmentation. Bateson's assumption that there is, what we may name a "*miscellaneous metamerism*," which, beginning in various parts, may lead on to true mesothelial metamerism is contrary to all morphological canons. That he considers this assumption necessary is another indication of the inherent weakness of his case.

So far as knowledge and discussion have brought us to-day, morphology has the choice between the two theories as to the origin of Vertebrates, the Appendicularia and the Annelid theory. A decision between these two alternatives, which shall be convincing to zoologists, is not at present possible, yet something, I think, may be determined as to the probabilities of the final choice.

D. The *Appendicularia theory* is the direct outcome of Kowalewski's embryological researches, and has been the subject of

general attention from zoologists for a quarter of a century past. The full and able defense of it by W. K. Brooks in his Salpa monograph is so recent and well-known that it will suffice to recall that the theory assumes that Appendicularia is near the ancestral pelagic type of the Chordata; that it gave rise to a form with multiple gill openings leading to Tunicates, and by the further acquisition of segmental structure leading to Amphioxus, and from this last on to the true Vertebrata. It must be carefully noted that by the Appendicularia theory the prime morphological importance and significance is attributed to the notochord and the branchial pharynx, and a secondary importance to the metamerism, while the Annelid theory reverses this and attributes prime importance to the metamerism, and secondary to the notochard and pharynx.

E. The *Annelid theory* was founded independently by Dohrn and Semper, and has always been supported in my opinion by far more numerous and sounder arguments than any other theory of Vertebrate descent, with possibly the exception of the Appendicularia theory. The fullest presentation known to me, of the Annelid theory, is that by Hugo Eisig in his monograph of the Capitellidæ. Those, who have had theories of their own to defend, have never attempted (ought we not say, have never dared tc attempt?) to confute the Annelid theory. Sedgwick, Bateson, Willey, Gaskell, Patten, Morgan and others have either left the subject alone, or at most, have spoken slurringly and in disparagement of it, so that there are literally no important direct criticisms against the theory known to me. I take this to signify that the writers in questiod have attacked the problem of the ancestry of Vertebrates not in a judicial but in a somewhat partisan spirit.

4. APPENDICULARIÆ VS. ANNELIDA AS THE ANCESTORS OF THE CHORDATA.

The most essential difference between the two theories under consideration is formulated above in the paragraph on the Appendicularia theory. The alternative of choice between the two theories hinges upon the interpretation of Amphioxus which we accept, i. e.:

1. Is Amphioxus a Tunicate becoming a Vertebrate by the acquisition of segments?

2. Is Amphioxus an Annelid becoming a Tunicate by the acquisition of a notochord and gill bearing pharynx?

Of course, we all understand that Amphioxus is probably not the exact transitional form, but merely the nearest living ally, known to us, of the transitional form.

5. FURTHER AFFINITIES OF AMPHIOXUS.

We have already maintained that the nearest affinities of Amphioxus are with the Tunicates. We now are confronted by the question: Are its affinities on the other side closer with A, the Vertebrates, or B, the Annelids? Here I believe we we must start with the decision to leave the pharynx and notochord out of consideration, because these structures are characteristic of the Chordata as a whole and not of the Vertebrata. But there are three sets of organs in Amphioxus which offer special peculiarities morphologically; these are the segments, the sexual organs and the excretory organs. Let us consider these in order.

1. *Segments.*—The mesothelial segments of Amphioxus arise with their cavities in open communication with the archenteron. Ray Lankester has advanced, and O. Hertwig and others have adopted, the hypothesis that the coelom of Vertebrates is an enterocoele, but hitherto, although special investigations have been made, no conclusive evidence has been found that in any true Vertebrate there is a clear resemblance in the early condition of the mesoderm to the disposition characteristic of Amphioxus. Traces of evagination of the mesoderm along the edge of the notochord, as required by the enterocoele theory, have been described in various Vertebrate embryos by a number of authors. These traces are so slight and so variable, that I believe they would be considered as meaningless variations in the grouping of cells, similar to those occurring in other parts of the same embryos, were investigators not unduly influenced by the preconceived theory. No investigator has claimed, so far as I am aware, that any Vertebrate exhibits segmentally arranged mesoder-

mic diverticula. In other words, even if Lankester's enterocoele hypothesis holds true of the Vertebrates, they would still show only a very remote resemblance with Amphioxus so far as the early stages of the mesoderm are concerned. But there is yet another important difference between Amphioxus and the Vertebrates, namely, in the former the segments are total, in the latter partial. In Amphioxus the whole of the mesothelium is segmented, in Vertebrates only the dorsal regions of the mesothelium are segmented, the ventral regions or walls of the splanchnocoele are not segmented. That there is an impressive similarity between the formation of the mesodermic bands in Amphioxus and Annelids was shown by the discoveries of Hatschek, and in both types the segments are total. It is perfectly clear, therefore, that the early history of the mesoderm indicates a much closer affinity of Amphioxus with the Annelids than with the Vertebrates. The existing differences may be explained by the assumption that the cavities of the segments are formed in Annelids after, in Amphioxus during, the morphological separation of the mesoderm from the entoderm.

2. *Sexual organs.*—The sexual organs of Amphioxus are confined to the branchial region, they are segmentally arranged and they are lateral of the excretory organs. Those of Vertebrates do not appear in the branchial region, they are not segmented and are mediad of the excretory organs. The view that the sexual organs are developed from segmental anlages has been advanced, and such anlages have been named gonotomes. I have shown elsewhere, that this view is based upon an incomplete consideration of the facts, and recently C. Rabl has fully confirmed my arguments. In Annelids the sexual organs are segmented, and are so disposed that we can understand how, from an Annelid ancestor, they may have given rise to the disposition found in Amphioxus, but, unfortunately, no careful study of such possible comparisons is known to me. Evidently a direct comparison of the sexual organs of Amphioxus with those of Vertebrates involve the most serious difficulties, while the comparison with Annelids promises well.

3. *Excretory organs.*—Boveri has shown that the excretory organs of Amphioxus are tubules which begin with a ciliated internal funnel and open through the ectoderm, and that their primitive arrangement is probably segmental; they are confined to the branchiate region. They differ, therefore, from the segmental organs of Vertebrates, which open into a longitudinal internal duct, and occur only behind the branchiate region. It is true that the attempt has been made to show that the duct is originally derived from the ectoderm, but the weight of evidence—see especially Price on Bdellostoma, and Rabl on Elasmobranchs—is at present against this view and in favor of the mesothelial origin of the duct. In Annelids segmental organs have the same arrangement as in Amphioxus, they open directly through the ectoderm, and they also may occur in that part of the body which, on the theory of Annelid ancestry, must have been evolved into the pharynx. The morphology of the excretory organs is thus readily perceived to indicate the Annelidian rather than the Vertebrate relationships of Amphioxus.

And now I ask, must we not conclude that Amphioxus is a Chordate nearly related to Tunicates, more remotely related to Vertebrates, and revealing in its organization important differences from other Chordata, which differences demonstrate a true kinship with Annelids? Now certainly no one would venture to regard Amphioxus as the ancestor of the Annelida, hence we are obliged to consider the Annelida as representing the ancestral type of the Cephalochorda, and, therefore, of all Chordata.

b. *The position of Appendicularia.*—The conclusion just stated involves the assumption, so far as now appears, that Appendicularia is not an ancestral but a secondary type. But Appendicularia much resembles a young stage of other Tunicates (free swimming larvæ), and we have learned to trust so much to the law of recapitulation (Haeckel's Biogenetisches Grundgesetz) that it is hard to believe that it does not govern Appendicularia, which would then be of the phylogenetic significance assumed by W. K. Brooks, whose argumentation is so strong, that even while defending the Dohrn-Semper theory, one must have in reserve readiness to change sides.

7. ORIGIN OF VERTEBRATA.

If the Annelids gave rise to the Chordata, we must consider the possible origin of certain anatomical characteristics of the Vertebrates, *sensu strictu.* Of such characteristics three are specially prominent, the general arrangement of the mesoderm, the pronephric duct, and the head with its arrangement of mouth, brain and eyes so unlike that in Annelids.

1. *The mesoderm* of Vertebrates, as already remarked, is characterized by having a large splanchnocoele and by being segmented only in its dorsal or epiaxial part. In Amphioxus the mesoderm is at first totally segmented as in Annelids, and later develops a splanchnocoele, and assumes in other respects an arrangement (of sclerotome and cutis plate) wonderfully comparable, according to Hatschek, with the vertebrate type of mesoderm. Amphioxus seems to really afford the key for the transition of the mesoderm from the Annelid to the Vertebrate type, and the occurrence of the actual transition ontogenetically renders it natural to advance the theory of the corresponding transition phylogenetically, thus obviating one of the former difficulties in the Annelidan theory of the origin of Vertebrates.

2. *The pronephric duct,* (often referred to as the Wolffian duct). This is a longitudinal epithelial tube, into which the segmental organs of Vertebrates open. Hatschek, in his paper on Polygordius, suggested that the segmental organs might acquire a direct secondary connection with one another; a series of such connections would make a longitudinal duct. Edward Meyer found such connections in adult Annelids, and other instances are known, see Willey's Amphioxus, 80–82. Price's observations on the Californian Myxinoid, and Rabl's on Elasmobranchs, tend to show that this is the actual origin of the duct. We no longer, therefore, need to take refuge in Haddon's or Boveri's hypothesis of the origin of the duct, since Hatschek's theory is sufficient and is supported by such actual evidence as we possess.

3. *Cephalic homologies.*—Up to this point we have dealt with knowledge and ideas derived from previous scientific publica-

tions, and have done little more than attempt to present the problem critically. As regards the head, however, even theories have hitherto failed us, and no hypothesis as to the evolution of the vertebrate head from the Annelidan type has been brought forward, which could not be shown to encounter insuperable objections, or at least which appeared insuperable to those who were opposed to the Annelidan theory. The suggestion most widely known and discussed is that the invertebrate mouth disappeared, the oesophageal ring with the neighboring ganglia formed the vertebrate brain, and a new vertebrate mouth was evolved, according to Dohrn, by the fusion of two gill clefts in the ventral median line.

I ask your consideration for an entirely different theory of the homologies of the vertebrate head. A morphologist must feel grave hesitation in assuming that such important structures as the main lateral eyes and as the oral evagination should have totally disappeared during the evolution of Vertebrates, for they are an advancing type, not a degenerate type losing its organs. He must also feel grave hesitation in assuming that main lateral eyes have been evolved in the Articulates and Vertebrates without any genetic relationship. It is natural, therefore, to test the assumption that the articulate eyes and vertebrate eyes are phylogenetically homolgous, it being, of course, understood that the comparison excludes ocelli, accessory eyes in Annelids and the pineal eye of Vertebrates. We note at once that the visual sensory apparatus in both cases is epithelial in type, and is derived immediately from the ectoderm, and further that in both cases the sensory apparatus is directly connected with nervous substance, in Articulates the so-called optic ganglion, in Vertebrates the nervous tissue of the retina, and finally that in both cases there runs from the optic apparatus a fibre tract, which is not a nerve, but a commissure, that is to say, a differentiation of a part of the central nervous system itself; this tract we call a part of the oesophageal commissure of the Articulate, and the optic nerve of the Vertebrate. We are thus lead to the supposition that eyes and optic nerves represent in Vertebrates the main eyes, the supra-oesophageal ganglia, and the oseopha-

geal commissures of the Articulates. Can this supposition be upheld? I do not think that the invagination of the optic vesicles in Vertebrates will be said by any morphologist to invalidate the comparison. At first it seems to reduce the so-called "brain" of Articulates to insignificance, but as we know that this brain, or more correctly, supra-oesophageal ganglion, is mainly a visual ganglion in Articulates, and it is by no means a great degradation for it to appear merely as a retina in Vertebrates. We must not be influenced by the misuse of the word "brain" applied to Invertebrates. But how can two diverging vertebrate eyes, which form a Y with the main nervous system, be the same as a complete nervous ring around the oesophagus? This difficulty is met by assuming that the eyes and optic ganglia fail to meet in the median line in front. In connection with this assumption, remarks upon two points are necessary. *First*, in Invertebrates with oesophageal nerve rings, the supra-oesophageal ganglia do not apparently arise in the median line, but as two symmetrical anlages, which subsequently meet in the median line. I recall that this is the method of development in Lumbricus (E. B. Wilson), Arachnids and Insects (Patten), Crustacea (Kingsley), and in Crepidula (Conklin). Other observations could be cited.[3] Hence we have only to believe that in the Vertebrates the separation is maintained and increased. *Second*, there should be found a cause for the separation of the eyes (supra-oesophageal ganglia) in the Vertebrates. This is given by the growth of the vertebrate brain, which is enormous and precocious. If the brain acquires great size, the eyes are correspondingly separated. It is evident that our assumption makes the brain homologous with the suboseophageal ganglia probably plus several ganglia of the ventral chain, the remainder of the ventral chain becoming homologous with the spinal cord.

Next as to the invertebrate mouth. If the eyes and optic nerves represent the oral nerve ring, we must look for the invertebrate mouth between the eyes. The invertebrate mouth

[3] That the observations are conflicting as to the paired origin of the Articulate brain is well known, but the theory that the median Scheitelplatte evolved into the Articulate brain encounters difficulties which seem to me very serious.

is an ectodermal invagination, which has a direct or acquired open communication with the entodermal cavity or archenteron. Is there such an evagination in the Vertebrates? Unquestionably there is, and a large conspicuous and well known one, which gives rise to the olfactory pits and to the hypophysis proper. It is important to recall that the naso-hypophysal invagination is single, and we may homologize it with the Annelid ectodermal stomodaeum. It seems to me a very significant fact that in the lowest Vertebrates (the Myxinoids) the naso-hypophysal invagination opens through the entoderm into the pharynx. This connection, which has been known but the significance of which has remained obscure for three-quarters of a century, may, under the supposition we are considering, be interpreted as the survival of the Annelidan condition. In other Vertebrates (Petromyzon and Gnathostomes) the opening mentioned does not recur, although contact between the ectoderm of the invagination and the pharyngeal entoderm can, it is claimed, be observed in certain forms (Accipenser, etc). To avoid confusion, it must be pointed out that the hypophysis is only a part of the invagination, and neither it nor the general invagination are connected with the neuropore; the hypophysis of Vertebrates is, therefore, not homologous with the neuroporic pit, which Willey and others have erroneously named the hypophysis of Amphioxus and Tunicates.

What is the actual vertebrate mouth? If we interpret the naso-hypophysal invagination as the homologue of the Annelidan mouth-gut, we find that the oral invagination is separated from the hypophysal by a fold, which becomes the upper jaw in Cyclostomes, and which is present in the embryos of all Vertebrates. (In Anmiote embryos this fold forms a ridge between Seesel's pocket and the hyphophysis). It, therefore, still remains necessary to look upon the vertebrate mouth as a new acquisition. It seems to me that, under the new conception of the head, Dohrn's hypothesis is more promising than before. But beyond this I can add nothing to the explanations of the vertebrate mouth previously attempted.

The brain arose, by the present supposition, from the ganglia of the "ventral" chain of Annelids. Even in Articulata, in the Crustacea and Arachnida, we encounter a tendency of the thoracic ganglia to make a brain. If the brain did thus arise, its enlargement would cause, as we see in the Vertebrate embryo, the brain to expand forwards, and since there is no corresponding growth on the hæmal side, this expansion would cause the projecting head to bend over towards the hæmal (Annelid dorsal, or Vertebrate ventral) side, and thus carry the mouth into a new position, and at the same time prevent the eyes and praeoral ganglia from meeting in the median line and force the visual organs into lateral positions. We thus reach a natural and simple explanation of the difference between the Annelid and Vertebrate head. The conception brought forward is favored by the conditions observed in Myxine and Petromyzon, for the eyes in these forms are small, in Myxine without a lens. The large size of the eyes is a subsequent acquisition of the Gnathostomes. By my hypothesis we should expect the evolution of the brain to precede that of the eye in Vertebrates. Finally, the vertebrate brain, by our supposition, was segmented originally throughout, and that this idea is correct is indicated by the trend of recent opinions formed by investigators of the cephalic neuromeres, and the morphology of the cephalic nerves in the Vertebrata.

The series of considerations outlined justify the hypothesis presented, namely, the Vertebrate head was evolved from the Annelid head. The three principal factors in this evolution were: 1. The formation of the vertebrate brain from several of the post-oral ganglia of the ventral chain. 2. The preservation of the ectodermal mouth-gut (Vorderdarm) of Annelids as the naso-hypophysal invagination. 3. The conversion of the visual apparatus and supra-oesophageal ganglia, which are prevented from fusing in the median line by the enlargement of the vertebrate brain, into the vertebrate eyes (retina), the oesophageal commissures persisting as optic nerves.

If this hypothesis be ultimately verified and shown to be in full accordance with the facts, it will obviate those difficulties

of the Dohrn-Semper Annelid theory of the origin of Vertebrates, which at present are the most serious.

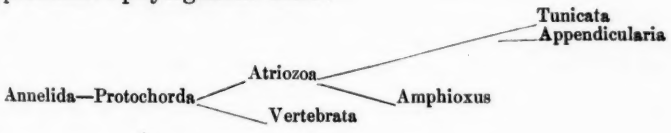
One more point, the cephalic homologies advocated increase the morphological importance of the eyes, and further emphasize the divergence between Amphioxus and Vertebrates, and makes the absence of lateral eyes in Amphioxus more significant even than before, and affords a further justification for the union of Amphioxus with the Tunicata.

I have presented the subject in the merest outline, for almost every sentence might be expanded to a paragraph, often into a chapter, without making the treatment exhaustive, but I hope enough has been said to make the argumentation clear and to justify it.

The origin of Vertebrates is one of the most difficult and obscure problems which at present occupy the general attention of zoologists. I am well aware that my own studies have concentrated upon the embryology of Vertebrates, and that it is difficult, perhaps, impossible, for one man to have a first hand mastery through his own observations of all the evidence which must decide the final solution. I have expressed, therefore, not convictions, but probabilities, and must certainly reserve the right to adopt other conclusions than those above advocated. The rival theories have sometimes taken on a very positive tone, and their advocates have referred to the Dohrn-Semper theory as a thing of the past, discarded and quite disproved. This attitude is regrettable, and I hope, by showing that much can still be said in favor of the origin of Vertebrates from Annelids, to contribute towards a sober and judicial discussion of an important and interesting topic.

8. CONCLUSION.

It remains only to present the following simple and comprehensive phylogenetic table:—



THE LIMITS OF ORGANIC SELECTION.

By Henry Fairfield Osborn,

OPENING A DISCUSSION BEFORE THE SECTIONS OF GEOLOGY AND BOTANY AT DETROIT.

The object of this paper is to set forth certain views as to the limits of the supplementary natural selection hypothesis recently proposed by Prof. James Mark Baldwin, Prof. C. Lloyd Morgan and myself as "Organic Selection."

The line of thought which led me to Organic Selection was as follows: The distinction between the *ontogenic* and *phylogenic variation* was drawn in my mind in 1894,[1] because it was evident in the current researches upon variation by Weldon, Bateson and others, and in the line of reasoning followed by Cope, Ryder, Scott, Osborn and other Neo-Lamarckians that the importance of such a distinction was being overlooked. There are three main types of variation: First, *fortuitous congenital* variations which are the temporary and transitory fluctuations around a mean. Second, *ontogenic variations*[2] which are the departures from normal or typical development arising during *ontogeny*; they include all the effects of the reaction of the individual to new or disturbed conditions of life which rise in the course of individual growth and may disappear with the death of the individual; the mooted question whether ontogenic variations are or are not heritable does not affect their distinctness. Third, *phylogenic variations*, also congenital, which belong in the phylum, as observed principally in fossil series; they are stable and inheritable departures from ancestral types towards a new type; they correspond with the "mutations" of Wagner and Scott, i. e., they are departures

[1] Alte und Neue Probleme der Phylogenese. Ergeb. d. Anat. u. Entwick., Merkel u. Bonnet, III Band, 1893, pp. 584–625.

[2] Prof. C. Lloyd Morgan has proposed to apply the word "*modification*," variously used by Cope, Bailey and other authors, to what is above described as "*ontogenic variation*" and to restrict the term "*variation*" to congenital variation. This excellent suggestion subserves clearness, and should be adopted by all writers.

from ancestral types which have become permanently established. They constitute the main *evidence* for determinate variation and as a consequence determinate evolution.

In every analysis of variation these distinctions are of profound importance, because every adult organ we study (whether with Weldon it is the frontal measurement of a crab or, with Cope and Tornier, the articular facet of a bone), may be an exponent either of constitutional, phylogenic, or stirp factors, or of new environmental and ontogenic factors, or of the fortuitous or chance elements in development, or finally of all three factors combined.

In March, 1896, the application of this distinction to the problem of the *causes* of "determinate variation" was pointed out by myself in course of a discussion in the New York Academy of Sciences (p. 141) as follows: "For example, if the human infant were brought up in the branches of a tree as an arboreal type instead of as a terrestrial, bi-pedal type, there is little doubt that some of the well known early adaptations to arboreal habit (such as the turning in of the soles of the feet, and the grasping of the hands) might be retained and cultivated; thus a profoundly different type of man would be produced. Similar changes in the action of environment are constantly in progress in nature, since there is no doubt that the changes of environment and the habits which it so brings about far outstrip all changes in constitution. During the enormously long period of time in which habits induce ontogenic variations, it is possible for natural selection to work very slowly and gradually upon predispositions to useful correlated variations, and thus what are primarily *ontogenic variations* become slowly apparent as *phylogenic variations* or congenital characters of the race. Man, for instance, has been upon the earth perhaps seventy thousand years; natural selection has been slowly operating upon certain of these predispositions, but has not yet eliminated those traces of the human arboreal habits, nor completely adapted the human frame to the upright position. This is as much an expression of habit and ontogenic variation as it is a constitutional character. This fact, which has not been sufficiently emphasized before, offers an explana-

tion of the evidence advanced by Cope and other writers that change in the forms of the skeletons of the vertebrates first appears in ontogeny and subsequently at birth in phylogeny."[3]

On April 13, 1896, I formulated the matter in a paper before the Academy entitled "A Mode of Evolution Requiring neither Natural Selection nor the Inheritance of Acquired Characters," which has since appeared in *Science*. Professor Baldwin, of Princeton, and Professor Lloyd Morgan, of University College, Bristol, had at the same time, independently reached the same hypothesis, and Professor Baldwin has aptly termed it "Organic Selection." Both writers have presented valuable critical papers upon it, including in *Science* and *Nature* a complete terminology for the various processes involved. I concur entirely in their proposal to restrict the term *variation* to congenital variation, to substitute the term "*modification*" for ontogenic variation, and to adopt the term "*Organic Selection*" for the process by which individual adaptation leads and guides evolution, and the term "*orthoplasy*" for the definite and determinate results.

The hypothesis, as it appears to myself is, briefly, that *ontogenic adaptation is of a very profound character, it enables animals and plants to survive very critical changes in their environment. Thus all the individuals of a race are similarly modified over such long periods of time that, very gradually, congenital variations which happen to coincide with the ontogenic adaptive modifications are collected and become phylogenic. Thus there would result an apparent but not real transmission of acquired characters.*

It is a subsidiary question whether this hypothesis is new, and a more important one whether it is true and constitutes a distinct advance towards the discovery of the unknown factors of evolution, or a satisfactory substitute for the Lamarckian theory of transmission of acquired characters.

[3] A writer in the *Fortnightly Review* has given a somewhat extreme illustration of the difference between ontogenic and phylogenic progress when he says: "Man is still, mentally, morally and physically, what he was during the later Palæolithic period." "The Artificial Factor in Man." Fortnightly Review, October, 1896.

It appears that the idea involved in Organic Selection is by no means a new one, it is formulated, for example, but not with especial stress or clearness by Weismann in his *Romanes Lecture* of 1894, as shown in the following selections from pages 11–17 (italics our own):[4]

"Hermann Meyer seems to have been the first to call attention to the adaptiveness as regards minute structure in animal tissues, which is most strikingly exhibited in the architecture of the spongy substance of the long bones in the higher vertebrates. . . . . But the direction, position and strength of these bony plates are by no means innate or determined in advance: they depend on circumstances. . . . . It is not the particular adaptive structure themselves that are transmitted, but only the quality of the material from which intra-selection forms these structures anew in every individual life. Peculiarities of biophors and cells are transmitted, and these may become more and more favorable and adaptive in the course of generations if they are subject to natural selection. . . . . *Intra-selection effects the special adaptation of the tissues to special conditions of development in each individual.* . . . . Let us take the well-known instance of the gradual increase in development of the deer's antlers, in consequence of which the head, in the course of generations, has become more and more heavily loaded. . . . . It is by no means necessary that all the parts concerned—skull, muscles and ligaments of the neck, cervical vertebræ, bones of the fore-limbs, etc.—should simultaneously adapt themselves by *variation of the germ* to the increase in the size of the antlers; *for in each separate individual the necessary adaptation will be temporarily accomplished by intra-selection—by the struggle of parts—under the trophic influence of functional stimulus.* . . . . But as the primary variations in the phyletic metamorphosis occurred little by little, the secondary adaptations would probably, as a rule, be able to keep pace with them. *Time would thus be gained till, in the course of generations, by constant selection of those germs the primary constituents of which are best suited to*

[4] The Effect of External Influences upon Development. The Romanes Lecture, 1894. London, 1894.

one another, the greatest possible degree of harmony may be reached, and consequently a definite metamorphosis of the species involving all the parts of the individual may occur. . . . ."

What appears to be new therefore in Organic Selection is, first, the *emphasis* laid upon the almost unlimited powers of individual adaptation; second, the extension of such adaptation without any effect upon heredity for long periods of time; third, that *heredity slowly adapts itself to the needs of a race in a new environment along lines anticipated by individual adaptation, and therefore along definite and determinate lines.* This hypothesis, if it has no limitations, brings about a very unexpected harmony between the Lamarckian and ultra-Darwinian (Weismannian) aspects of evolution by mutual concessions. While it abandons the transmission of acquired characters, it places individual adaptation first and fortuitous variation second as Lamarckians have always contended, instead of placing survival conditioned by fortuitous variations first and foremost as Selectionists have contended. If true, it is thus a compromise between the pure Lamarckian and pure Darwinian standpoints in which the concessions are about equal. And if true it gives us at least a partial explanation of determinate variation which Lamarckians have recently contended for, and Darwinians have strenuously denied.[5]

Professor Alfred Wallace has recently endorsed this hypothesis in a review of Professor Morgan's work, "Habit and Instinct," in the March, 1897, number of *Natural Science* in the following language: "Modification of the individual by the environment, whether in the direction of structure or of habits, is universal and of considerable amount, and it is almost always, under the conditions, a beneficial modification. But every kind of beneficial modification is also being constantly effected through variation and natural selection, so that the beautifully perfect adaptations we see in nature are the result of a double process, being partly congenital, partly acquired.

[5] See Osborn: Cartwright Lectures, Present Problems in Evolution and Heredity, 1891. Also, "Is Variation Definite or Indefinite?" American Naturalist, 1889.

Acquired modifications thus helps on congenital change by giving time for the necessary variations in many directions to be selected, and we have here another answer to the supposed difficulty as to the necessity of many coincident variations in order to bring about any effective advance of the organism. In one year favorable variations of one kind are selected and individual modifications in other directions enable them to be utilized; in Professor Lloyd Morgan's words: 'Modification *as such* is not inherited, but is the condition under which congenital variations are favored and given time to get a hold on the organism, and are thus enabled by degrees to reach the fully adaptive level.' The same result will be produced by Professor Weissman's recent suggestion of 'germinal selection,' so that *it now appears as if all the theoretical objections to the 'adequacy of natural selection' have been theoretically answered.*" (Italics our own.)

Alfred Wallace thus accepts this new phase of the natural selection theory and maintains that it removes the last of the theoretical objections to the adequacy of that theory. I do not wish to be understood as taking such a sanguine view; I rather maintain the conservative position which I have held for many years in regard to the adequacy of both the Lamarckian and Darwinian theories.

Moreover, in course of discussion of this subject with my friends Professors Lloyd Morgan, Baldwin and Poulton, a very fundamental difference of opinion becomes apparent; for they agree in believing that the power of plastic modification to new circumstances, or what the Rev. Dr. Henslow has termed "self-adaptation," is in itself a result of natural selection. In other words they hold that natural selection has established in organisms this power of invariable response to new conditions, which, in the vast majority of cases is essentially adaptive. I disagree with this assumption *in toto*, maintaining that this plastic modification is, *so far as we know* an inherent power or function of protoplasm. This view, I understand, is also held by Driesch, E. B. Wilson, T. H. Morgan and probably by many others. The only cases in which self-adaptation may be demonstrated as produced by natural se-

lection are where organisms are restored to an environment which some of their ancestors experienced. We can then imagine that the adaptive response to the old environment is something which has never been lost as in the well known reappearance of the pigment in flounders.

It may be urged against the Morgan, Baldwin, Poulton views that the remarkable powers of self-adaptation, which, in many cases are favorable to the survival of the individual, are in many cases decidedly detrimental to the race, as where a maimed or mutilated embryo by regeneration reaches an adult or reproductive stage. It is obvious that reproduction from imperfect individuals would be decidedly detrimental, yet from the view taken by the above authors such reproduction would be necessary to secure the power of plastic modification for the race.

It is certain, that at the present time, one of the surest and most attractive fields of inductive research, leading towards the discovery of the additional factors of evolution or what I have elsewhere called "the unknown factor," is in experimental embryology and experimental zoology. If we could formulate the laws of self-adaptation or plastic modification we would be decidedly nearer the truth. It appears that Organic Selection is a real process, but it has not yet been demonstrated that the powers of self-adaptation which become hereditary are only accumulated by selection. They may possibly be accumulated by the inheritance of acquired modifications as Lamarck supposed.

Furthermore, another difficulty which I find with the completeness of the Organic Selection hypothesis is identical with that which almost from the outset made me hesitate in regard to the completeness of the Lamarckian hypothesis, namely, many structures, such as the teeth, which exhibit no power of self-adaptation or plastic modification during life, which are, in fact, rendered decidedly less effective instead of more effective by use and habit—these structures, I repeat, show precisely the same determinate and definite variation and consequent evolution as that which is exhibited in plastic and self-adaptive structures. This being the case, it is clear that "Organic Selec-

tion" leaves a very large field of determinate evolution entirely uncovered and unexplained, and there remains a *tertium quid* which requires further investigation. Determinate evolution in these non-plastic structures at present strikes me as part of the mechanical necessities of development, if I may so express it. That is, given a certain primitive form, there is only one route along which it can attain a certain end, provided the intervening stages are mechanically effective. It is some such law of mechanical necessity as this which out of the conical type of reptilian teeth has evolved first the triconodont type, the tritubercular, and finally the multitubercular, and from these main stages have arisen sub-stages which are repeated and independently acquired over and over again in different branches of the mammalian class. This is not an explanation, or a theory, it is a fact yet to be understood.

Organic Selection constitutes a distinct advance, and is, at least, a very useful working hypothesis, but it is by no means the conclusion of the whole matter, as Alfred Wallace maintains. We must persevere in our analysis of life processes as revealed in living organisms and in fossils with a perfectly open mind, perhaps for many decades, perhaps for another century, before we reach final conclusions in regard to the complex processes of evolution.

Columbia University.

THE GEOLOGICAL CONGRESS IN RUSSIA.

By Charles Palache.

The Seventh International Geological Congress assembled in St. Petersburg during the first week of September, 1897. The Congress was notable among the meetings of this organization for the large number in attendance. It will certainly be memorable, to such of its members as took part in them, for the extent and interest of the excursions planned in con-

nection with it and for the warm-hearted hospitality tendered them in every part of the broad empire which they visited.

The program of excursions, issued early in the year by the Committee of Organization, was carried out in all its essential details. Moscow was the starting point of the first excursion to the Urals preliminary to the Congress, and consequently it was that city which most of the excursionists made their first objective point on entering the country. We found wherever we crossed the border that our membership tickets made the passage through the Custom House easy, although they did not replace our passports which we had often to show. Our railroad passes too, were accepted without question, and the most courteous treatment greeted us on every hand.

At Moscow we found the Bureau of the Congress established in the halls of the University, and here and in the parlors of the Continental Hotel, frequent gatherings of the excursionists took place during the three days we were in the city. The excursions in the vicinity of Moscow were of moderate interest geologically, the greatest attaching to that which visited the richly fossiliferous middle Carboniferous beds of Miatchkowo on the bank of the Moskwa River. On the other hand, the city itself with its wholly oriental character in architecture, coloring and street life, offered more than enough attractions to occupy the time at our disposal, and there were doubtless few who did not regret leaving this, the centre and fountain-head of the Russian national life.

About one hundred and thirty-five persons took part in the Ural excursion, of whom nine were ladies. A special train of thirteen cars was our means of transport and place of abode for the first three weeks of the trip, while a restaurant train of box cars, provided with tables and chairs, preceded us and furnished forth our meals. If there were numerous discomforts associated with this style of living, one could not but remember that there was no precedent for or experience of such an excursion as ours in the remote regions which we visited, and criticism was disarmed by the conditions.

From day to day excursions of various kinds were made along or away from the general line of the railroad which was

our base of operations. A steamer trip from Samara, on the Volga—a walk along the railroad, fifteen miles from Acha to Miniar, giving a fine section of the Carboniferous and Devonain strata—a two day's wagon trip to the iron mines (limonite) of Bakal—a visit to the foundry at Simsk and its charming environs—such were a few of the earlier excursions which, besides showing interesting geological sections, gave us a good opportunity to become acquainted with the customs and mode of living of the people of the region. And at every mine and foundry, in many of the towns and even at the railway stations, we found the heartiest welcome awaiting us, great concourses of people who looked upon us with undisguised curiosity but evident good will, receptions by the local authorities, and numerous lunches and banquets of the most lavish description. It is difficult indeed, to express the feeling of growing wonder which all shared at the continuous ovation that greeted us on all sides as we made our progress through these mountainous regions, seemingly so little calculated to afford such entertainment as we found. Still, harsh as it may seem to criticise in such a case, it must be confessed that there was too large a share of our time devoted to social functions, and we might have seen many additional localities of geologic interest had we not been compelled by our kind hosts to arrange our movements in accordance with their too frequent hospitalities.

The guide book of the excursions was prepared in the form of separate pamphlets for various localities or regions, written by the men who were best acquainted in each and who were to be our leaders. The descriptions were generally good, though often lacking in details; the sections and illustrations were satisfactory, though the mine sections rarely corresponded with the visible exposures. The accompanying geological map of Russia, scale 1:6,300,000, reduced from the larger map published by the Geological Committee in 1892, scale 1:2,520,000 served as a very convenient means of orientation.

In the region between Moscow and Oufa where our leader was Nikitin, we saw horizontal or slightly disturbed strata ranging from Cretaceous downward to middle Carboniferous.

At Oufa, at the foot of the Ural Mountains, we entered a zone of openly folded paleozoic rocks including the Devonian series, the folds becoming rapidly more compressed and the disturbances greater as we advanced into the mountains. At Slatoust we encountered the first crystalline schists, considered by Tschernyscheff our leader, on stratigraphic evidence which but few of the party considered conclusive, to be metamorphic Devonian.

Within this band of schists or on its borders, is a group of mineral localities which have produced many interesting and beautiful specimens obtained by the efforts of many engineers of the Russian Mining Bureau through a long series of years, and now in large part preserved in the cabinet of the Mining Academy in St. Petersburg. These minerals all appear to be contact products between clay slates and limestones and massive eruptive rocks of the character of diorite or peridotite. One of the best known and most typical of these occurrences is the Achmatoff mine. Here, on a more or less chloritic matrix, were found beautiful crystallizations of garnet, epidote, pyroxene, vesuvianite, such titamium minerals as perofskite, titanite and ilmenite, zircon, apatite and various members of the chlorite group.

Passing eastward still across the Ural divide, we entered a region of gneisses and granitic rocks. The day spent at Miass, in the Ilmen Mountains, under the joint leadership of Karpinsky and Arzruni, was replete with interest. The Ilmen Mountains are a subordinate range of the Ural chain composed largely of eleolite-syenite, classical under Gustav Rose's name of *miascite*. This rock is well exposed, is rich in a variety of minerals and offers numerous interesting problems to the petrographer. The most notable minerals here collected, chiefly from the pegmatitic facies of the rock, were nepheline, cancrinite and sodalite, zircon, apatite, ilmenite and biotite in huge plates. In the pegmatite veins traversing the neighboring gneiss and granite, we saw a very different group of minerals including albite and microcline, topaz, zircon and euclase and samarskite, pyrochlor and other rare earth minerals.

At Miass, also, was the first of the gold placers which are worked, by what seemed crude and primitive methods, in various parts of the Urals.

At Tcheliabinsk, the eastern limit of our excursion, as well as at Beresof, near Ekaterinburg, we saw gold-bearing quartz veins, the former only recently explored, the latter with extensive workings dating back many years.

At Ekaterinburg, the principal city of the Ural region, we we were hospitably entertained by the Ural Society for Natural Science, and were shown an interesting exhibit of the products of numerous local establishments for the cutting of gems and semi-precious stones.

Continuing northward our next halt was in the busy mining town of Njni-Taghilsk. Within this district are the extensive iron mines (magnetite) of Wyssokaia and Blagodat; the copper mine of Mednoroudiansk, famous for its former production of the malachite so prized in Russian decoration; largely worked deposits of manganese ore, and the platinum placers at Platina. This last metal appears to occur in the peridotitic rocks which constitute the bed rock of the region; their decomposition sets it free, so that it may be won by placer washing.

Again crossing the water-shed of the Urals and descending rapidly to the plain we reached Perm on the Kama River, where we left our train for a roomy and comfortable steamer on which for three days we floated down the stream to its confluence with the Volga. Numerous excursions on the banks made us acquainted with the Permian series, including the upper, Permo-triassic division, the so-called " etage tartarien." Turning up the Volga we halted at the old tartar city of Kazan, where we were entertained by the university and later by the city. And the end of the fourth day from Perm found us at Njni-Novgorod. A day was spent here seeing the fair, and then we took train directly for St. Petersburg, where we found the quarters previously assigned us in the many hotels of the city or in the large dormitory of the university, where some fifty of the members were located. The hundred and twenty members of the Finland excursion reached St. Peters-

burg the same morning (August 28th) by steamer, reporting a most interesting week's trip, during which they had enjoyed quite as lavish hospitality as had been the lot of those in the Urals. They were under the guidance of Sederholm, and saw much glacial geology in addition to the old crystalline formations of western and southern Finland.

The formal opening of the Congress took place the afternoon of Sunday, August 29th, in the hall of the Zoological Institute of the University, which was well filled by a large and brilliant audience. The Honorary President of the Congress, the Grand Duke Constantine Constantinovitch, presided and opened the session with an address of welcome. It was followed by similar addresses by the Princess of Oldenburg, President of the Imperial Society of Mineralogy, and by the Minister of Agriculture, Ermoloff. Renevier, President of the preceding Congress, announced the officers named by the council, the American Vice-Presidents being Marsh, Emerson, Emmons and Frazer. The address of the President, Karpinsky, was chiefly occupied with a brief statement of the questions to come before the Congress, and after a resumé by Tschernyscheff, Secretary-General, of the work of the Committee of Organization in arranging for the Congress and excursions, the session came to a close.

Of the eight hundred and fifty names which appeared in the official list of the members of the Congress, upwards of six hundred were recorded as in attendance. Despite this very large membership the actual number at the meetings was very small, rarely more than one hundred being present. But the adjoining hall where numerous exhibits were arranged was always occupied by a throng of members, showing very clearly that here as generally in such meetings, it is the personal intercourse that is desired by the members rather than the formal discussions.

The subjects which it was desired to have specially brought before this Congress, as announced in an early circular of the Committee of Organization and as stated in the President's address, were as follows:

1. Shall stratigraphic nomenclature be based upon an artificial or upon a natural classification.

2. Establishment of rules governing introduction of new terms in stratigraphic nomenclature.

3. Adoption of definite principles of classification of rocks and of petrographic nomenclature.

The discussion of the first question showed clearly the opinion of most of the geologists present that the accepted artificial classification was the only admissable one, its abandonment for what must necessarily as yet be a somewhat vague and ill-defined substitute, being certain to result only in a state of confusion in the science.

Discussion of the second proposition centered upon papers presented at an early meeting by Frech, Breslau, Ueber Abgrenzung und Benennung der geologischen Schichtengruppen, and by Bittner, Vienna, Vorschläge für eine Normiring der Reglen der stratigraphischen Nomenclatur.

The propositions of these writers as modified and accepted by the Congress amounted to little more than a formal statement of the ordinary practice of geologists with regard to new names. They were in brief as follows:

1. Introduction of a new stratigraphic term into the international nomenclature, should be based only on a well determined and peremptory scientific necessity, should be accompanied by full description of deposits to which it is applied, and should be founded on facts observed on more than a single exposure.

2. A name applied to any deposit in a definite way is not to be used in another sense.

3. Date of publication determines priority of terms.

4. In giving new names to minuter stratigraphic subdivisions, it is desirable to take paleontological characteristics as a base. Geographic names should only be used in default of the former, or for series of importance containing numerous paleontological horizons.

5. Names etymologically false or badly formed are to be rejected or corrected.

One afternoon meeting was devoted to subjects of petrographic character, and in order to facilitate discussion of the general questions in that department, a special meeting of pet-

rographers, to the number of over fifty, was held, Zirkel presiding. The discussions were animated, centering upon classification, but resulted in the following almost unanimous expression of opinion, which was presented through the council to the Congress:

"It is not desirable, in view of the present rapid development of the science of petrography, to attempt to establish definite principles of classification of rocks by a resolution of the Congress.

"To attain the simplification of petrographic nomenclature demanded by geologists, it is necessary to define with greater precision than has yet been done such general terms as are required in geological mapping."

A resolution presented by Brögger expressed the view that it was desirable and probably practical to establish a international journal of petrography devoted chiefly to reviews and abstracts of the current literature. This resolution excited considerable debate, but was finally adopted and transmitted to the Congress with the request that a committee be appointed to ascertain the feasibility of the plan. The committee named consists of fifteen men, the American members being Iddings and Pirsson.

Papers of interest presented in this department were by Walter, Jena, Versuch einer Classification der Gesteine auf Grund der vergleichenden Lithogenie, and by Loewinson-Lessing, Dorpat, Note sur la Classification et la Nomenclature des Roches Eruptives.

Walter attempts a general classification, his basis being the recognition of primary and of secondary characters in rocks, of which the first alone determines the place in the system. Thus every metamorphic rock, however altered by "secondary characters," is grouped under the rock from which it is derived. He makes four main groups, Mechanical, Chemical, Organic and Volcanic Rocks.

Loewinson-Lessing bases his classification of the eruptive rocks wholly on their chemical composition, expressed in terms of the "oxygen equivalent," and of the proportions of the various oxides.

The Congress endorsed strongly a proposition by Androussoff, Dorpat, for the establishment of an International Floating Institute, or laboratory, for the study of Oceanography, to be supported by various Governments in place of the isolated expeditions sent out for this purpose from time to time by different ones.

The invitation of the French geologists to hold the next session of the Congress in Paris in 1900 was accepted. A bulletin was distributed showing the proposed excursions to Brittany, to Normandy, and, after the session, to the Central Plateau and the French Alps.

A sad incident of the Congress was the sudden death in St. Petersburg of one of its members, Spendiaroff, Dorpat, who had taken part in the Ural excursion and was to have been a leader during a part of the Caucasus excursion. At the closing session of the Congress it was announced that the father of the deceased had presented a sum of money to commemorate his son, the interest to be awarded as a prize for the best paper on a stated subject at successive meetings of the Congress.

Numerous festivities and excursions were a part of the week's program at St. Petersburg. The Tsar received in audience at his palace of Peterhof, a small number of the more distinguished representatives of each country. At a later day, but in his absence, the whole Congress visited the palace and beautiful grounds, journeying down the harbor by steamer and enjoying a luncheon in the magnificient imperial dining hall.

Another day was devoted to a visit to the cataract of Imatra in Finland, a hundred miles north of St. Petersburg, where a sumptuous banquet was served in a pavilion erected for the purpose on the edge of the thundering torrent.

Among the entertainments in St. Petersburg, the most notable were the reception by the Grand Duke Constantine at his palace and that by the Mayor and city officials at the City Hall. Many pleasant reunions took place at the German Club which was placed at the disposal of the members during the session. The museums and collections of the city were made easy of access through special open hours, and their manifold

attractions doubtless were to blame for the frequently slim attendance at the meetings.

The close of the session was marked by the departure of a large proportion of the members for Moscow, the starting point of the three excursions which journeyed by different routes toward the Caucasus and the Crimea. Upward of four hundred people participated in these excursions which promised so many and so varied interests.

SOME UNWRITTEN HISTORY OF THE NAPLES ZOOLOGICAL STATION.

All American biologists are familiar with the Zoological Station at Naples, either through having enjoyed its unrivalled facilities or from accounts of it which have been published again and again in the journals, both scientific and popular, of the two worlds. It is, beyond question, the greatest establishment for research in the world. But while it occupies this position to-day, and while its history since it first threw its doors open to the investigator is a part of the history of biology, the station has an unwritten history which is extremely interesting, especially since it shows, in strongest light, the indefatigable industry and resourcefulness of its founder and director, Dr. Anton Dohrn, in overcoming obstacles of every sort, many of which would have discouraged a man of less persistence. On the evening of August 10, 1897, Dr. Dohrn told the students at the Marine Biological Laboratory at Woods Holl, some of the difficulties which he encountered before the station was ready for students. His talk is summarized in the following account, his own words being used in some cases.

After apologies for possible linguistic mistakes due to the use of a foreign tongue and for the prominence of himself in what he had to say, Dr. Dohrn continued by asking his audience to imagine a young privat-docent of the University of Jena, with rather more money than he well knew how to spend; with more time than he knew how to use, but with a

strong desire to do something of lasting benefit for science, and you have an idea of one of the factors in the foundation of the Naples Station. This young man had already opened a small laboratory in a modest way at Messina, but it was far from meeting his ideals. Location, equipment, support, were not the best imaginable.

On his return journey to Germany several points were visited and critically examined as to availability for a large station, and Naples seemed in every respect to promise best, but the great problems were how to get the proper location, how to obtain the necessary influence and support. For a young doctor with a reputation to make and with only the money granted by an indulgent father, both obstacles were rather serious.

One day in the late sixties, while returning from Berlin to Jena, an idea came which promised, at least in part, to remove some of the difficulties; it was to combine with a station for research, an aquarium for the entertainment of the people. Both Hamburg and Berlin had aquaria which were most successful, why should not Naples be equally favorable? So back to Naples he went at once.

At this time Naples was making great preparations for a Maritime Exposition, and it was thought that the aquarium project would work in well with this, and that, the exposition over, the building could continue and develop its scientific side. The director of the exposition was interviewed; he favored the plan, and arrangements were at once made by which the aquarium was to be inaugurated as a part of the exposition, the young privat-docent in his eagerness and enthusiasm, agreeing to meet all bills, while the director was to assume charge of the plans and the ordering of the material and equipment. Soon Dr. Dohrn saw his mistake. The director planned as he liked, ordered as he liked and soon bills came in for things totally different from anything which Dohrn himself would have ordered. So there was a break in the arrangements, and the privat-docent was back exactly where he was before—location and support still to be obtained.

He drew up a few plans, designed an elevation of the building he desired, and with these he aroused the interest of Professor Panceri of the University of Naples, who advised a consultation with the authorities of the city, who had then in progress plans for a park or garden on the shores of the beautiful bay. They, too, were attracted by the project and designs, and since they were to have the aquarium free of expense to the city, the very location desired was granted, the grant, however, being coupled with several conditions. Some of these restrictions were almost laughable. Thus no one was ever to sleep in the building, and when it was pointed out that the proprietor of the restaurant in the garden spent the night in his building, the reply was made that this was an exception. Again, it was stipulated that there should be no kitchen connected with the station, the reasons for this proviso being that if there were a kitchen it would be so easy to convert the structure into a hotel, and it would never do to have a hotel in the gardens. The other restrictions were far more serious, and Dr. Dohrn felt that he could not subscribe to the agreement in the shape it was presented to him by the Naples council. At this juncture he applied to the Italian Government, which then had its seat at Florence, and in a few days received the characteristic advice, "subscribe to everything, and then do as you please," advice which later was to make trouble for him.

Then came the Franco-Prussian war and the station plans were set aside for a time, for Dr. Dohrn was ordered to his regiment, and he went to France to take part in that struggle. When the war was over he was soon back in Naples. An architect was engaged and the station and its aquarial adjunct seemed on the straight road to accomplishment. But this bright prospect was soon darkened. The architect, like others of his class, had his own ideas of what a zoological station should be like, although up to the moment of his engagement he had never seen such an establishment, nor had he ever dreamed of one. At last he returned with his plans, Dr. Dohrn glanced at them, saw that they were totally unfitted for a zoological station and pushed them aside on the table, whist-

ling, as he did so, the closing phrases of Beethoven's Ninth Symphony, a reminiscence of a concert of the evening before. The architect rushed from the room in rage, and shortly his representative called upon Dr. Dohrn to make arrangements for a duel. The matter was finally settled, the architect received a thousand francs for his unusable sketches and another was installed in his place.

At last actual building was commenced and slowly the walls went up. Dr. Dohrn's father was dead and the patrimony was all invested in the new building. Three times the projector was at a loss as to from where the money for the week's payment was to come, and at as many times were friends found who aided in the undertaking. At last all resources were exhausted, friends had given all they could, Dr. Dohrn's private fortune had gone into brick and iron and marble. The thought came, this station is to be educational in its ends, cannot the German Ministry of Instruction be induced to aid by giving the 30,000 marks necessary to finish the building.

So there followed another trip to Berlin. Dr. Dohrn called on the Minister, told his story and asked if the Government could make such a grant and received for an answer, "no." As Dr. Dohrn tells it, the Minister made no attempt after this refusal to close the interview, but seemed to wait as if the answer were not final, but as if he were still open to conviction upon proper argument, and so the Doctor told a story to the effect that after the battle of Sedan he obtained a leave of absence to attend the meeting of the British Association for the Advancement of Science. When in England a friend commented upon the Franco-German war and the successes of the Germans, to the effect that this was a beginning of the end of the German pre-eminence in intellectual matters; that now Germany would develop into a military power and in all other respects would fall behind. "For years" said he, "we have been translating books from the German, in the next ten years you will be translating English books into German." The moral of the story was appreciated and the Minister promised that if Dr. Dohrn could obtain the endorsement of the

Berlin Academy of Sciences the desired grant would be forthcoming.

So Dr. Dohrn immediately interested the sympathies of three of the most prominent members of the Academy, Helmholz, DuBois Reymond and Virchow. They promised their aid and assured him that the desired endorsement would be forthcoming, and, rejoiced by the good prospects, back he went to Naples, only to find new troubles which he had never suspected.

In the agreement with the municipality it was stipulated that the station building should not exceed a certain heighth, but the architect whose discharge has already been mentioned, had gone to the officials and had shown them that the walls were nearly a metre higher than was permitted, a fact which was really the result of this architect's own act. He was taking his revenge.

There was trouble immediately. The papers were filled with denunciations of the young German who had thus dared to violate the stipulations of the city, and many there were in the council who demanded that the whole building should come down. Certainly the prospect looked blue enough, but there was more to follow. On that very Christmas eve word was received from Berlin that the Academy had refused to endorse his petition, and that the ministry consequently refused to grant the necessary money. The grounds for non-endorsement were that the aquarium gave the enterprise a mercenary rather than a scientific aspect, and, again, that Dr. Dohrn had as yet done no scientific work that gave him the standing necessary for the head of such an establishment. Dohrn immediately wrote his friends, but was in Berlin before his letters. He personally called upon every member of the Academy, and such were his representations that he received the desired endorsement at the next meeting of the Academy, and the grant of funds immediately followed.

There still remained the matter of the quarrel with the city of Naples. In this Dr. Dohrn thought that possibly diplomacy could be made to play a part, and so the German Crown Prince was interested in the matter and shortly the Govern-

ment at Berlin began to exert its influence upon the powers of Italy. Long before this, however, Dohrn was back in Naples, where, upon his return, matters were even more unpromising than upon his departure. Soon the results of diplomacy began to make themselves felt, while Dr. Dohrn used his own influence with the officers of the city. Soon he had won to his side a member of the clerical party, and in a short time every obstacle on the part of the municipality was removed.

The building was at last completed and the time came to turn the water into the huge aquarial tanks, which have since proved such an attraction in the station. Here was more trouble. In Naples when a water-proof surface is needed it is the custom to use, instead of cement, a kind of volcanic earth beaten tightly together with rammers. So the builders used this for the bottom of the tanks regardless of the fact that they were to withstand salt water instead of fresh, and this some feet, instead of a few inches, in depth. The water poured through these bottoms in streams. This part of the work had to be done over again.

At last the station was opened and students began to come. There was a demand for such a place and the station fulfilled the demands made upon it as no other institution could. Yet it lived from hand to mouth, and many were the desirable features which must be omitted from the lack of funds. The whole was started, but it was far from the ideal station which its director had in mind. It was at this juncture that the German Embassador to the Italian Government visited the station. He was pleased with all that he saw and impressed with the needs of the enterprise. "I think" said he, "that the difficulties are not insuparable," and shortly after, as a result of his representations to the German Government, backed as they were by the endorsement of Virchow, DuBois Reymond and Helmholz, the Reichstag granted an annual subsidy of $40,000 to the Naples Zoological Station.

WIND RIVER AND BRIDGER BEDS IN THE HUERFANO LAKE BASIN.[1]

By Henry Fairfield Osborn.

In 1888 Professor R. C. Hills, of Denver, announced his very important discovery of tertiary beds in the Huerfano River basin of southern Colorado. He contributed three papers to this subject in the Proceedings of the Colorado Scientific Society in 1888, 1889 and 1891, and finally divided the beds into three series, namely:

Huerfano Series (Eocene)	Huerfano Bed	Bridger Group	3,300.
	Cuchara Beds		300.
	Poison Canon Beds	Lower Eocene (Green River Wasatch and Puerco)	3,500.

The identification of the Huerfano Beds proper was made by means of a large collection of fragmentary fossils. The identification of the lower beds was upon stratigraphic evidence only, Professor Hill observing that they underlay conformably the upper beds. The essential features of his conclusions were as follows:

(1) That the Huerfano series of 3,300 feet are equivalent to the Bridger or middle eocene, and the Cuchara and Poison Cañon series are probably equivalent to the lower eocene.

(2) At the close of the Laramie period a great anticlinal axis arose to the east and southeast of the Wet Mountain Range and east of Spanish Peaks, forming the eastern border of the lake, extending fifty miles north and south, and from ten to twenty miles east and west.

(3) The eruption of the laccolithic Silver Mountain and Spanish Peaks was subsequent to the Upper Lake Deposits of Bridger age. Hence these deposits are found upon the slopes of Spanish Peaks.

(4) The drainage of the Huerfano Lake was to the north through the Wet Mountain Valley.

[1] Read before the American Association for the Advancement of Science at Detroit.

In May, 1897, the writer accompanied by Dr. J. L. Wortman, made a brief reconnaissance of this basin, and came to the following conclusions; differing from those reached by Professor Hills:

(1) That west of the Huerfano Cañon the variegated marls, clays, soft shales and sands aggregate only 800 to 1000 feet in thickness, are nearly horizontal in position, and constitute alone the true Huerfano Lake deposits. They may be positively divided into Upper Beds, equivalent to the Bridger. From the observations and conclusions made in the basin there are also undoubtedly Lower Beds, equivalent to the Wind River.

(2) That the so-called Cuchara and Poison Cañon Beds are unconformable with the Huerfano and are of older age, probably of Cretaceous, as partly determined by the presence of Baculites in the Poison Cañon section, which was selected by Professor Hills as typical.

(3) That the eastern boundary of the Huerfano Lake is partly indicated in the present cañon of the Huerfano River; that this boundary extended to the south so as to include the base of Silver Mountain toward the Cuchara Divide; that it lies from three to seven miles west of the 'anticlinal axis' described by Professor Hills, and that, therefore, the Huerfano Lake deposition did not extend as far to the east or south as the Spanish Peaks.

The geological features of these conclusions can hardly be dignified by the term "A theory of the Huerfano Lake," for they were formed during a hasty survey of this basin; while Professor Hill's results certainly deserve the deliberate consideration of a prolonged survey. In fact this basin with its volcanic disturbances and eruptions presents a fascinating problem in the geology of tertiary times. Among the Bridger forms discovered were many portions of the skeleton of *Titanotherium*, beside remains of *Hyrachyus*, *Palæosyops*, *Microsyops*, *Calamodon*, *Stypolophus* and *Pachynolophus*. This region is peculiar in the absence of *Uintatherium*. In the Lower Beds are found teeth and limb bones of *Coryphodon*, *Lambdotherium*, *Oxyæna*, *Pantolestes* and other Lower Eocene forms, probably of Wasatch age.

The writer is greatly indebted to Professor R. C. Hills for his very full information in regard to the topography of the basin, and for assistance and advice in connection with the trip.

PECULIAR ZONAL FORMATIONS OF THE GREAT PLAINS.

By Frederic E. Clements.

The traveller through the sand hills of Nebraska has often brought to his notice the striking way in which nature has marked, as though for all time, the fields and groves which once dotted the country. Such areas are always most conspicuous, because of the strange contrast between their sharply marked dark green and the thin, brown vegetation of the sand hills. Frequently the waste is a flaming mass of the western sunflower, *Helianthus petiolaris*, in which case it is distinctly visible for several kilometers. In many localities, such wastes have existed for more than a score of years, and, instead of diminishing in any respect, become each year more and more accentuated.

The elevated prairies and tablelands, which are so typical of the Great Plains before the latter rise into the foot hills of the Rocky Mountains, are characterized by a floral covering monotonous in the extreme. Trees and shrubs are entirely absent, and undershrubs are present only in peculiar alkaline areas, and in "bad lands." The color-tone of the floral covering is green only for one or two spring months: after the first of June, it becomes a uniform straw color, stretching in all directions to the horizon. The two principal formations of the high plains of western Nebraska are the *Stipa comata* formation, and the peppergrass-cactus formation. Rarely, the former is traversed by a sandy zone several kilometers wide and 20–30 kilometers long, characterized by the *Artemisia filifolia* formation. An individual of *A. filifolia* regarded alone is scarcely green, but the mass of individuals, by contrast with

the straw-colored Stipa, give a dark green tone to the formation. The floral covering, composed of the Stipa and the peppergrass-cactus formations, is seamed here and there with old, abandoned trails, and what may by courtesy be called roads. These have necessarily originated extremely narrow, but often very long, minor tensions between the original floral covering and the invading roadside flora. Along the roads travelled at present, these tensions have attained expression in a narrow zone at either side. In some instances, this zone consists of dwarf individuals of *Helianthus petiolaris*, in others of *Malvastrum coccineum*, or of *Gutierrezia sarothræ*, in still others, of dwarfed plants of *Salsola tragus*, closely appressed, but conspicuous on account of the unusual deep green color. Not infrequently, Malvastrum and Salsola intermingle to constitute the zone. A common result of such a tension in the Stipa formation is to accentuate the size, and density of growth of the Stipa to such a degree that the formation is bordered along the road by a most conspicuous zone composed wholly of its own facies. In the same formation, the Stipa zone is sometimes suppressed, and its place is occupied in part by scattering, silver-purple bunches of *Artemisia frigida*. In trails a long time abandoned, the sterile strip between the bordering zones disappears, being encroached upon and vanquished by the plant constituting these zones. Such trails then become not only striking members of the floral covering, but not altogether canny features of the landscape as well. From the base of Scott's Bluff, a deeply sunken trail extends far toward the Wild Cat mountains in the southeast, marked over sun-browned plain and ridge by an endless band of dark green, due to the dense bunches of *Gutierrezia sarothræ*. The stage-road from Harrisburg over the hills and undulating plains of Kimball county is flanked on either side by a trail, once well-worn, but now densely crowded with the silver-purple tufts of *Artemisia frigida*. These floral land-marks run parallel to the stage route for perhaps a half-score of kilometers, then swinging abruptly to the southwest, they pass on over valley and ridge, disappearing in the one only to reappear upon the other, until the eye refuses to follow further.

It is impossible to determine how long such subruderal formations have persisted. The size and thoroughness of establishment of Gutierrezia and Artemisia would indicate occupation for several decades. After a long period, however, it usually happens that *Stipa comata,* driving out the subruderal inhabitants, reconquers these trails. It is significant that the dark patches of *Opuntia humifusa,* or *O. polyacantha* so characteristic of this formation, never reappear with the Stipa. But even the speargrass is unable to resist the modifying influence of the trail, and its abnormally tall stems and compact growth find easy interpretation in connection with the ruts and ridges on which it grows. The old California trail is the most interesting example of this. Throughout the upper valley of the North Platte in Nebraska, this historic overland route is marked by such a zonal formation. From the base of Scott's Bluff, the California trail, first travelled more than three-score years ago, and abandoned for over a score of years, "angles" southeastward in a broad band of innumerable ruts, painfully insistent in their matted cover of bleaching grass-stems.

THE CRICKET AS A THERMOMETER.

By A. E. Dolbear.

An individual cricket chirps with no great regularity when by himself and the chirping is intermittent, especially in the day time. At night when great numbers are chirping the regularity is astonishing, for one may hear all the crickets in a field chirping synchronously, keeping time as if led by the wand of a conductor. When the numbers are so great, the resting spells of individuals are unnoticed but when the latter recommence they not only assume the same rate but the same beat as the rest in that field. The crickets in an adjoining field will have the same rate, that is, will make the same number of chirps per minute, but with a different beat as one may easily perceive by listening.

The rate of chirp seems to be entirely determined by the temperature and this to such a degree that one may easily compute the temperature when the number of chirps per minute is known.

Thus at 60° F. the rate is 80 per minute.

At 70° F. the rate is 120 a minute, a change of four chirps a minute for each change of one degree. Below a temperature of 50° the cricket has no energy to waste in music and there would be but 40 chirps per minute.

One may express this relation between temperature and chirp rate thus.

Let T. stand for temperature and *N*, the rate per minute.

$$T.=50+\frac{N-40}{4}$$

For example. What is the temperature when the concert of crickets is 100 per minute?

$$T.=50+\frac{100-40}{4}=65°.$$

EDITOR'S TABLE.

—One of the most important advances based upon scientific researches is now under discussion in Boston. The Associated Boards of Health of Massachusetts now advocate the licensing of every person engaged in the milk business, the licenses only to be granted after the thorough inspection of the locations of the business and the sources of the supply, even to an examination of the cows. Within the past year several severe epidemics of typhoid fever in and near Boston have been traced to milk supplies, and the very source of infection found. Only a few years ago milk was looked upon as the very safest drink and food for mankind, but one has only to consider the facts brought out by Prof. W. T. Sedgwick in his report upon milk to the Associated Boards of Health to see that we must sooner or later come to some governmental supervision in this matter, for as the case now stands in our cities, milk is about the most dangerous substance that enters our houses.

—Every one who reads the foreign journals is frequently gratified by the praise they accord to our government publications. Typo-

graphy, illustration and the matter itself all come in for commendation. There is, however, one department of our government with which fault must be found in this respect—the Department of Agriculture. Here the fault lies in the multiplicity of serials issued. It would seem that in this department each head clerk considered it necessary to issue his own publication, and, in many cases, several series of publications. The result is that in the present decade there have issued from this department about one hundred series of documents, so that students and librarians have great difficulty in keeping track of them. Then, too, these serials are unduly padded, the truly valuable matter which occasionally appears in them being buried among a mass of valueless material, apparently prepared from a spirit of rivalry between the different bureaus and divisions.

The following list of publications is probably not complete, but it is published here for two reasons: first to show the senseless extent to which this division into series has been carried; and, second, as an aid to librarians in arranging these publications and noting deficiencies.

The Department of Agriculture issues the following publications which are not distributed among the separate bureaus: Circulars, Miscellaneous Circulars, Farmers' Bulletins, Special Reports, Miscellaneous Special Reports, Reports of the Secretary, Year Book—a total of seven.

From the separate divisions appear the following:—

Bureau of Animal Industry: Circulars, Circulars of Information, Bulletins, Special Bulletins, Annual Reports.

Division of Soils: Bulletins.

Division of Agricultural Soils: Bulletins.

Division of Agrostology: Circulars, Bulletins.

Division of Biological Survey: Circulars, Bulletins.

Division of Botany: Bulletin (octavo), Bulletin (quarto), Circulars, Contributions from the U. S. National Herbarium, Special Bulletins, Illustrations of North American Grasses, Report of the Botanist.

Division of Chemistry: Bulletins, Circulars, Report.

Division of Entomology: Circulars, Circulars (second series), Bulletins, Bulletins (new series), Periodical Bulletins, Special Bulletins, Technical Series, Reports, Insect Life.

Division of Forestry: Circulars (octavo), Circulars (quarto), Bulletins (octavo), Bulletins (quarto), Reports upon Forestry, Report of the Chief of the Division of Forestry.

Division of Garden and Grounds: Report.

Division of Microscopy: Report, Food Products.

Division of Ornithology and Mammalogy: Bulletins, Reports, North American Fauna.

Division of Pomology: Bulletins, Circulars, Report of the Pomologist, Report of the Assistant Pomologist, Special Reports.

Division of Publications: Circulars, Reports.

Division of Records and Editing: Report.

Division of Statistics: Circulars, Crop Reports, Miscellaneous Reports, New Series Reports, Special Reports, New Series Miscellaneous Reports, Miscellaneous Series Bulletin, Miscellaneous Series Reports, Reports of the Statistician.

Division of Vegetable Pathology: Bulletin, Circular, Report, Journal of Mycology.

Division of Vegetable Physiology and Pathology: Bulletin.

Fibre Investigation Series.

Library (octavos).

Library Bulletin (quarto).

Office of Experiment Stations: Circulars, Experiment Station Bulletin, Experiment Station Record, Miscellaneous Bulletin, Bulletin, Reports.

Office of Irrigation Inquiry: Bulletin.

Office of Road Inquiry: Circular, Bulletin.

Section of Foreign Markets: Circular, Bulletin.

Seed Division: Report.

Silk Section: Circulars, Bulletins, Reports.

Weather Bureau: Annual Summary of New England Weather Service, Bulletin, Bulletin of New England Weather Service, Lake Storm Bulletin, Monthly Weather Review, Report of Ohio Weather and Crop Service (octavo), Ohio Section of Weather and Crop Service (quarto), Report of the Chief, Weather Crop Bulletin, Report of the North Dakota Weather and Crop Service, Circulars of Information.

This list is, we think, sufficient to support our contention that multiplication of serials in the Department of Agriculture has been carried to an absurd extent. It is high time that the Secretary call a halt and give the "Division of Records and Editing" a blue pencil and the authority to use it.

General Notes.

GENERAL BIOLOGY.

Reactions to Stimuli in Paramecium.[1]—The plan of the author has been to study the reactions of one organism so completely and exactly that we may gain a good idea of its daily activities. Phenomena such as these are to be explained. When a large number of Paramecia are transferred, together with a bit of decaying vegetable matter, from the culture jar to the glass slide and covered with a properly supported cover-glass, we see the Paramecia at first uniformly distributed, a few minutes later beginning to gather about the decaying particle, and soon all accumulated there. Some minutes later the Paramecia begin to disperse, but are always sharply confined within an ever extending circumference. If an electric current is now passed through the water, the infusoria swim towards the cathode, but do not pass the circumference. We seek an explanation of the aggregation of the infusoria, their subsequent dispersion and their limitation by an invisible boundary, even when urged to pass it by the electric current.

The results of a series of experiments which cleared up one difficulty after another in a manner very interestingly described are as follows: The beginning of the accumulation of the Paramecia is due to thigmotaxis. An individual, hitting the solid body by chance, stops, perhaps starts back and whirls on its axis, then settles against the object and remains there. Others do likewise; thus the accumulation begins. No response occurs to smooth hard bodies. The close application of the Paramecium to the surface and the gliding over it are the results of the peculiar reactions of the cilia induced by the stimulus. But the rapid and complete aggregation cannot be accounted for alone on the ground of thigmotaxis. The author finds that *Paramecia are attracted by a not too strong concentration of carbon dioxide.* The CO_2 produced by the thigmotactic individuals serves to lure the others. But after the crowd has become very dense the CO_2 becomes so strong in their vicinity that the Paramecia are repelled from the region of the solid body and begin to disperse. They do not scatter widely into the culture

[1] H. S. Jennings. Studies on Reactions to Stimuli in Unicellular Organisms. I. Reactions to Chemical, Osmotic and Mechanical Stimuli in the Ciliate Infusoria. Jour. of Physiology, XXI, p. 258–332, May, 1897.

fluid, however, for they are repelled by the latter also, but they keep in a zone of weak concentration of CO_2. The chemotactic movements of Paramecium were seen also in their repulsion by strong solutions of all acids, including CO_2, by all alkaline solutions, to which category the culture fluid belonged, and to certain neutral salts and organic compounds. Towards other organic substances, e. g., sugar, glycerine, urea, Paramecia is indifferent. Tonotaxis plays no important part in the normal activities of the organism. The reason why the infusoria are not forced beyond the circumference by the electric current is that they are less strongly electrotactic than chemotactic.

The following weighty conclusion is now drawn: Since infusoria are negatively tactic to their native fluid and positively tactic to the unadvantageous CO_2, negative or positive taxis is not necessarily an adaptive movement, is not always determined by its advantage to the species.

PALÆONTOLOGY.

Archegosaurus.[1]—The results of this preliminary paper are based on the rich material of Archegosaurus contained in the "Kgl. Museum für Naturkunde" and the collection of the "Kgl. geologische Landesanstalt" in Berlin. The archegosaurs are preserved in clay-geodes, and in spliting these the bones are generally broken. After the bones had been removed with chisels or fine steel-needles, a mixture of gelatine and glycerine was poured over the plates and very good reliefs of the skeleton were thus produced.

Jäckel intends to write a monograph on Archegosaurus and gives only the more important results. He commences with the skull, and afterwards discusses the vertebral column, the limbs and the dermal skeleton.

The skull.—In the palatal region he finds some differences from the statements so far given. There exist series of teeth on the inner sides of the vomers and palatines, which show essentially the same arrangement as in the Labyrinthodontia. Larger teeth are placed between the anterior ends of the choanæ, and behind these. The choanæ are very much longer than in the Labyrinthodonts. This elongation is certainly in relation to the anterior extension of the muzzle of Archegosaurus.

[1] Jäckel, Otto. Die Organisation von Archegosaurus. Zeitschr. deutsche Geol. Ges. Jahrg., 1896, Heft 3, p. 505–521, fig. 10.

The teeth of both the inner and outer rows are separated by considerable spaces from each other and are less crowded than in the Labyrinthodonts.

The outer side of the lower jaw of *Archegosaurus decheni* is figured. It consists of four sculptured bones. Following E. Fraas,[2] he names the upper element outside of the articular face, the articular, but he doubts whether it belongs to the endoskeleton. This element is the supraangular, the angular is correctly determined, and the "infradentale" is one of the splenials.[3]

The vertebral Column.—There are said to be 25 presacral, one sacral and over one hundred caudal vertebrae. Jäckel believes that there is uncertainty about the morphology of the vertebral column of the Stegocephalia and the higher vertebrata.

Among the Rachitomi he distinguishes four elements: 1. The paired upper arches [neural arches Baur] which in Archegosaurus unite dorsally into a spinous process [neurapophysis Baur]; 2. The paired upper Pleurocentra (Interdorsalia Gadow), which are intercalated between the upper ends of the Hypocentra; 3. The paired lower Pleurocentra, *which in the tail separate themselves from the upper Pleurocentra,* (Interventralia Gadow, Hypocentra pleuralia). 4. The unpaired Hypocentrum which in the tail may split into two centres of ossifications.

First I may mention that the name *Intercentrum*, first introduced by Cope,[4] antedates Gaudry's[5] name *Hypocentrum* five years.

Cope says "The basal portions of the chevron bones are continued throughout the greater part of the Vertebral column in the Permian Genera Clepsydrops, Metarmosaurus and Epicordylus [Eryops], forming intervertebral elements to which I have given the name *intercentra*" —"The free elements of the cervical series of some reptiles are probably the same."

The name Pleurocentrum was introduced by Gaudry[6] in 1879; in the

[2] Fraas, Eberhard. Die Labyrinthodonten der Schwæbischen Trias. Palæontographica Bd. XXXVI, p. 73, Aug. 24, 1889.

[3] Baur, G. Uber die Morphologie des Unterkiefers der Reptilien. Mit 4, Abbildungen., Anat. Anz. Bd., XI, Nr. 13, Dec. 21, 1895, p. 410–415.

[4] Cope, E. D. The Homology of the Chevron Bones. AMER. NATURALIST, May, 1875, p. 319 (Published, April 22d).

[5] Gaudry, Albert. Les Enchainments du Monde Animale dans les Temps Géologiques Fossiles Primaires. Paris, 1883, p. 273, fig. 273, A. B C.

[6] Gaudry, Albert. Les Reptiles de l'époque permienne aux environs d'Autun. Bull. Soc. Géol. d. France. (3) Tome VII, 1878–1879, Paris, 1879, p. 68, and p. 65 in explanation of fig. 7.

same paper he designated, Cope's intercentrum as "Pièce inférieure du centrum." I do not believe that there has been any uncertainity about the morphology of the vertebral column of the Stegocephali and the higher Vertebrates. The whole question was definitely solved. Everybody is convinced that the pleurocentra of the Rachitomi represent the centra of the higher Vertebrates; and that the intercentra are homologous to the intercentra of the Sphenodontidæ, Geckonidæ, Uroplatidæ, Pelycosauria. These intercentra support in the tail the chevron bones, or hæmal arches; both are firmly united.

In some papers published this year by Dr. Hans Gadow[7] and Prof. Alexander Götte,[8] it is maintained, that the intercentrum of the Rachitomi forms the centrum of the Amniota; Prof. Jäckel comes to the same conclusion. Gadow and Götte have never examined any fossil Stegocephali or Pelycosauria and Jäckel, by some unfortunate calculation, has completely misunderstood the true nature of the archegosaurian vertebral column.

I shall show that this new opinion is in opposition to all morphological facts. In Archegosaurus the rib-heads are articulated below with the intercentrum, behind with the pleurocentrum and above with the pointed base of the neural arch. In the tail the lower arches (hæmapophyses, chevron bones) are united with the intercentra. In the Pelycosauria: Clepsydrops, Dimetrodon, Naosaurus, Embolophorus, Theropleura and others we have exactly the same condition. There are very well developed intercentra between the centra, which are suturally united with the neural arches. The capitulum the lower part of the rib-head is articulated to the intercentrum, the tuberculum to the anterior portion of the centrum and the base of the upper arch. In the tail the lower arches (hæmapophyses, chevron bones) are united with the intercentra.

According to Gadow, Götte, Jäckel the Intercentrum Cope, of the Rachitomi is homologous to the centrum of the Amniotia. There is no doubt, that the intercentrum plus hæmal arch (chevron) of the Pelycosauria is homologous to the intercentrum plus hæmal arch (chevron), of Archegosaurus; therefore the intercentra of the Pelycosauria are centra, according to the authors named above. Therefore, the Pelycosauria have two centra. This of course is absurd, therefore the

[7] Gadow, Hans. On the evolution of the vertebral column of Amphibia and Amniota, Philos. Trans. Roy. Soc. London, vol. 187 (1896), B. pp. 1–57. London, 1896, June 16.

[8] Götte, Alex. Ueber den Wirbelbau bei den Reptilien und einigen anderen Winbelthieren. Zeitschr, wissensch. Zool., vol. LXV, p. 343–394.

intercentra of the Rachitomi are not centra, but true intercentra, as everybody has believed so far.

We only need to examine some of the Stegocephalia in which the ossification of these elements is more advanced. In old specimens of *Eryops megacephalus* Cope, the pleurocentra are closely united to the posterior base of the neural arches; the intercentra are placed between the pleurocentra below and do not reach the neural arches. Only the first intercentrum is connected with the neural arches of the first vertebra the atlas, forming an atlas-ring as in all Amniota. How is it possible that this first intercentrum can be a centrum? The pleurocentra of Eryops are homologous to the centra of the Amniota. The rachitomous condition is the most primitive one. Before Jäckel, I described the condition in Archegosaurus as follows:[9] If we examine the vertebral column of Archegosaurus, we see that the notochord is still developed and that in the dorsal region each body of the vertebra consists of three parts, two lateral ones, the pleurocentra, and one inferior one, the intercentrum (hypocentrum). In the tail region we find even five elements; the two pleurocentra and below them two small hæmacentra Hay[10] (hypocentra pleuralia) and the intercentrum to which the hæmal arches (chevron bones) are attached."

There is very little doubt, that in the caudal vertebræ of the Rachitomi the hæmacentra, if they were present, formed with the pleurocentra a cartilaginous ring. In the precaudal region only the pleurocentra become ossified and support the neural arches. The intercentra were continued dorsad as cartilage, also forming a ring. In the tail the chevrons, lower arches, are united with them. This primitive condition is modified in two ways. First: the *intercentrum* increases in size, especially the lower portions, become broader, until they meet with each other. The *pleurocentra* become reduced, or confluent with the upper arch. The intercentra form the body of the vertebræ of the Labyrinthodontia, they are wedge-shaped, the notochord never passes through the centre, but is placed in an excavation at the upper free border of the intercentrum, or there is a small fossa at the upper posterior face of the atlas, or there may be a chordal foramen just below the upper border. From all this it is evident, that the "centra" of the *Labyrinthodontia* are *intercentra*. This opinion is now generally accepted.

[9] Baur, G. The Stegocephali. A Phylogenetic Study. With 8 Fig. Anat Anz. XI, Bd., N. 22. Mærz 20, 1896, p. 657–673.

[10] Hay, O. P. On the structure and development of the Vertebral column of *Amia*. Field Columbian Museum. Publication 5. Zoolog. Series. Vol. I, No. I, p. 40. Chicago, U. S. A. October, 1895.

The Labyrinthodonts reached a large size and became extinct at the end of the Trias.

The second modification of the rachitomous condition is seen in Cricotus Cope. Each vertebra consists of two fully ossified elements. The centra in the precaudal region completely support the neural arches, which have well-developed diapophyses. These diapophyses are placed on the neurocentral suture which is, however, completely obliterated. The centra are 15 mm. long, 25 mm. broad, and 25 mm. high; they are very deeply biconcave and notochordal. In front of this centrum is a complete intercentral disc, of the same breadth and heighth as the centrum, but only 8 mm. long; laterally at the posterior border it possesses a small process to which the capitulum of the rib is articulated. These flat intercentral discs have a very large notochordal foramen (4 mm. in diameter). In the tail the intercentral discs carry the chevrons. This condition has been called by Cope *embolomerous* and the suborder *Embolomeri*. It is evident that the *centra* are homologous to the pleurocentra. The Embolomeri became extinct in the Permian. Only two genera are known, Cricotus Cope and Diplovertebron. From the Rachitomi the Amniota developed. The pleurocentra formed the centra, and the *intercentra* were more and more reduced. Intercentra are present between all the vertebræ in the Pareiasauria (Cotylosauria) Pelycosauria, Rhynchocephalia, Geckonidæ, Uroplatidæ. In the Ichthyosauria, and Lacertilia they are confined to the anterior cervical vertebræ. In the Megalosauria, Iguanodontia, Pterosauria and Birds; the first intercentrum forms the lower piece of the atlas ring, and the second intercentrum is united with the centrum of the atlas (odontoid process) and the centre of the axis into one mass. In all mammals the first intercentrum always remains, forming the lower piece of the atlas ring, and in some mammals they are even present in the dorso-lumbo-sacral region, for instance in the Insectivora (Talpa, Erinaceus, Myogale) and in Atherura among the Rodents.[11]

I gave in 1886 a full historical account of the views on the morphogeny of the vertebral column of the Amniota from 1844, giving all the morphological, paleontological and embryological evidence.[12] Dr. Gadow certainly did not study this paper, for, after the quotation of it in his Literature, he puts in parenthesis "Extract, in German, of Cope's discoveries."

[11] Baur, G., l. c., p. 663.

[12] Baur, G. Uber die Morphogenie der Wirbelsäule der Amniolen. Biolog. Centralbl. Band VI, Nr. 11 and Nr. 12, Aug. 15, 1886, p. 322–342; 353–363.

After the discussion of the vertebral column, Jækel makes some remarks on the ribs, limb-skeleton and the dermal covering, and concludes his paper with a short summary of his results. I hope that Jækel in his final monograph on Archegosaurus will give up the absolutely unfounded opinion of the homology of the intercentrum of Archegosaurus with the centra of the higher Vertebrata.—G. B.

Reconstruction of Phenacodus primævus, the most Primitive Ungulate.[13]—This paper was accompanied by the re-mounted skeleton of Phenacodus and a wax model executed by Charles Knight. As originally mounted in Professor Cope's laboratory, the famous skeleton of *Phenacodus primævus* conveyed a very imperfect impression of its actual form and proportions. Several serious errors were committed by the restorer, the most important of which was the implanting of two of the cervical vertebræ in the tail. The author, therefore, considered it advisable to completely re-mount the animal, and this has been done by Mr. Adam Hermann and Mr. Martin, of the American Museum, at an expenditure of five months time. The animal is placed as nearly as possible in a natural position. It shows that the feet were not plantigrade, or soled upon the ground, but digitigrade, as in the tapir. The body is characterized by the great convexity of the back, characteristic of the Carnivora and of all the early ungulates. A further unguiculate feature is the great development of the hind quarters and of the tail. The disproportion between the hind and the fore quarters is heightened by the extremely small size of the head, containing a brain which was about the size of that of the opossum, as fully described by Cope.—H. F. Osborn.

BOTANY.

Observations on the Distribution of Plants Along Shore at Lake of the Woods.[1]—Professor MacMillan's paper upon the vegetation of the shores at the Lake of the Woods is the most important contribution to American phytogeography since the Metaspermæ of the Minnesota Valley of the same author. In this paper, as in the latter work, the elements of the flora have been determined with almost

[13] Read before the British Association at Toronto.

[1] MacMillan, Conway: Observations on the Distribution of Plants along Shore at Lake of the Woods. Minnesota Botanical Studies, I, 949, 1897.

exhaustive minuteness, and it is not too much to say that, for a large portion of the two regions in which this territory falls, Professor MacMillan's work upon the floral elements will be found to be substantially final. But in the present paper, the author has done much more. He has given a sketch of the shore and beach vegetation from the standpoint of ecology, and a résumé of the formations which it presents. Although several papers of minor importance have dealt with certain aspects of some ecological and formational problems, Professor MacMillan has undoubted right to the honor of being the first in the field in this country, if not indeed in the English tongue, with an account of extended research in these lines. Pointing this out with pardonable pride, he has offered his contribution as an incentive to others to undertake and to continue a sort of botanical work which stands in need of many investigators. The author is emphatically a man of ideas, as he has demonstrated in more than one department, and his contribution at this time, when investigation in this country is but beginning, is welcome and opportune.

At the outset, it should be noted that the author's standpoint is purely ecological, not phytogeographical. He is concerned solely with the physical environment of the plants of the locality, not with the biological environment that in so many instances proves of no less moment. It is not easy to make distinctions of this sort, since they depend largely upon the point of view of each writer. But, in general, the phytogeographer looks primarily at the floral covering as such. He studies, analyzes, and traces the development of the floral covering as a whole, and in restricted areas. For this purpose, he brings to bear his knowledge of the physical and biological environment and of the plants themselves as well, their morphological adaptations as shown in primary and secondary biological characters, modifications for purposes of duration, seed-production, dissemination and so forth. But the ecological phytogeographers appear to restrict themselves for the most part to the investigation of certain aspects of the relation of the floral covering to its physical environment. This investigation is, without doubt, of great importance, but the limitations of such a procedure appear clearly in the paper under review.

The discussion of the physical conditions prevailing upon the shores at Lake of the Woods is a masterpiece of analysis. Everything has been taken into account, and every modification which might affect the floral covering has been investigated and pointed out. This analysis has been made not only with reference to the more important factors —water-content, illumination and temperature, but also with reference

to all minor influences. That portion of the paper dealing with the physical conditions of the locality is the most important, since it suggests methods of procedure for other regions. It deserves study at the hands of all who are engaged with similar problems. But, at the same time, it must be said that for most purposes the analysis will be found too exhaustive. The perception of the relation of habitat and organism stands at the foundation of phytogeography; yet all of the physical factors are not of equal importance. Plants have more to do than merely to contend against physical conditions; they have to contend with other plants. Individuals contend with each other, vegetation form with vegetation form, floral element with floral element, and formation with formation; a struggle goes on everywhere in the floral covering that forces plants to adapt themselves to strange and even unwelcome environments. The influence of modifications due to this necessity is one that may never be overlooked. Extended investigation of a large territory is required to enable one to measure the relative importance of these factors, but it cannot escape notice that a number of species listed by Professor MacMillan are ruderal species of varied habitat, which may be met with in meadow, clearing, high prairie or sand hill, as well as in the situations where he found them. In fact, it is only by beginning at the other end, by studying the vegetation forms of a region and the accessory characters of this vegetation, and so reaching an understanding of the means by which plants are enabled to take possession of the soil and to maintain themselves there, that one can comprehend the really characteristic and dominant vegetation and arrive at an understanding of formations.

The discussion of the biological factors which determine the vegetative covering of the region, therefore, seems quite inadequate. An analysis of these biological factors made with the same minuteness as that of the physical factors, might not be necesssary, and yet it cannot fail to suggest itself to those who have been engaged in the study of like problems in other regions, that the former, in general, is likely to be the much more valuable, if carried beyond a certain point. The physiographical and climatological conditions are undoubtedly of controlling force while vegetation is establishing itself in a new area. Once established, vegetation reacts upon its environment and upon itself, and new forces have to be reckoned with. One would think that a more careful consideration of these and of the influence of vegetation forms and their distribution would have made such phenomena, as the occurrence of Celastrus and Parthenocissus in the back-strand rather than in the mid-strand (p. 979) much more clear.

It has been pointed out in a previous paper that the formation is a definite phytogeographical concept, determined by its statistic, vegetation forms and habitat groups. The determination, or better, the recognition of a formation is conditioned by the previous analysis of its constituents from these various standpoints. This fact was first appreciated by the Continental phytogeographers, and Drude was the first to carefully elaborate the formations of an entire flora in accordance with this principle. The fundamental nature of his work in this line renders his researches classic, and his conclusions superlatively final, for the present at least. As is well shown in his Deutschlands Pflanzengeographie, I, it is necessary to first comprehend the floral covering by a careful analysis of its floristic, vegetation forms and habitat groups, and then by even more careful synthesis, determine the formations thus pointed out. While the floral covering may be truthfully likened to a mosaic in which the various pieces are formations, it is not true that every distinguishable bit of difference is a formation. It must likewise be borne in mind that formations are by no means of fixed and absolute expression: they are at all times more or less plastic, manifesting modification, incipience, decadence, and most intimate relations with other formations.

It is, at present, so common to reproach European botanists, and particularly those upon the Continent, for their disregard of the results of American investigation, that it is odd to be obliged to call attention to American neglect of European research. Yet the article in hand manifests justs such a lack of acquaintance with the fundamental contributions of Drude. Drude's concept of the plant formation is undoubtedly to be regarded as the correct one. Measured by this standard, the great number of formations noted for a small area by Professor MacMillan fall in most cases to the rank of facies, or patches, or they are at best only incipient or decadent stages of real formations. The author has committed two serious mistakes in his elaboration of the Lake of the Woods formations: the first is lack of perspective, the second is too minute analysis. It is impossible to delimit formations accurately by studying the floral covering of a restricted area. Such limitation can be done intelligently and well only in a natural vegetation area; except in the rarest cases, in nothing smaller than a vegetation region. A formation must be studied in all its various stations and in its many phases, before final conclusions can be reached concerning its validity. From the lists given, the floral covering in the vicinity of the Lake of the Woods is composed of the most heterogenous elements, which have crowded together in peculiar fashion. Appreciа-

ting this fact, then, such a floral covering could be well comprehended only after the exhaustive study of the formations of the entire vegetation region. Had this been done for the present contribution, many of the so-called formations would have been referred to the real formations of which they are immature or incomplete expressions. As a concrete illustration, all of what Professor MacMillan terms surf-barrier formations are merely isolated facies and patches of certain water-plant formations common throughout the Allegheny and Prairie provinces.

In analysing too carefully, the author has noted everything distinguishable as a formation, a method which serves very well for the mere cataloguing of phytogeographical phenomena, but is one scarcely to be commended as affording a basis for work of much value. Upon many such points, one not familiar with the country studied is incapable of passing judgment, but, in some instances, the mistakes are apparent even to one not conversant with the particular floral covering. Front-strand, mid-strand and back-strand represent merely a more or less radially biologically symmetrical formation, the nucleus of which is found in the back-strand, from which the formation "shades out" toward the lake with accustomed zonation. As for strand pools, the three sorts are merely developmental aspects of one formation. The formation is best represented by the back-strand pool, which is simply the ultimate expression of the water plant formation represented in its earlier stages by mid strand and front-strand pools. Dune pools are likewise to be referred to the same formation. Soil shore formations are not at all peculiar to the Lake of the Woods; they are aspects of formations which occur over a large portion of the continent. Talus formations are such only in rare instances; they are usually incipient formations of very various types. It is perhaps unnecessary to point out further instances of such incorrect analysis. It only remains to remark the similarity between the many lists of species of different formations, and to call attention to the large number of meaningless ruderal and subruderal plants contained in them. Formational statistics to be of value must be arranged with reference to facies, principal species, secondary species, etc., and not in bald lists.

—Roscoe Pound,
Frederic E. Clements.

ZOOLOGY.

The Cæcal Appendices of the Orthopteran Mid-Gut.[1]—Continuing his observations on the glands connected with the intestine, Bordas points out that in all the Orthoptera, with the exception of the Forficulidæ and Phasmidæ, the well known cæcal appendices of the grasshopper, though varying in form from family to family, may be recognized. The presence or absence of the cæca corresponds more or less closely with different external morphological characters and allows the orthoptera to be divided into two sections. Moreover, the number of these organs, their disposition, their mode of insertion, the folds presented by their internal surface and especially the presence or absence of posterior diverticula, are characteristics by which a number of families may be recognized. For instance, the Mantidæ and the Blattidæ are provided with 8 intestinal cæca, while the Acridiidæ possess but 6, and the Locustidæ as well as the Gryllidæ possess but 2. From his observations upon numerous specimens, he concludes that a series may be distinguished in which paired appendices stand at one end and multiple appendices at the other, or in other words, that the Pseudophylliniæ connect the Acridiidæ with the Locustidæ.—F. C. K.

For many years one of the problematic structures of vertebrate morphology has been the hypochorda or sub-notochordal rod, a slender cord of tissue developed like the notochord from the entoderm and lying ventral and parallel to the larger and better known chorda dorsalis. Its phylogenetic origin, its function and its fate have been uncertain. In the last number of the Morphologisches Jahrbuch, Bd. xxv, are two papers dealing with the subject. In the first, Dr. K. Franz deals with this structure in the Teleostean fishes. His conclusions in brief are that the structure in question is entodermal, that at no time does it contain a lumen, and that it sooner or later entirely degenerates without contributing any elements to the longitudinal ventral ligament of the vertebral column, a view contrary to that of some observers, but in full accord with broader views of morphology. The second paper is by Professor Hermann Klaatsch, and deals with the morphological significance of the subnotochordal rod. He summarizes his conclusions as follows: The hypochorda is a common possession of the higher vertebrates inherited from their ancestors. It is the rudi-

[1] L. Bordas: Compt. Rend. Acad. Sci. Paris, CXXIV, (1897), 376–378.

ment of an organ still functional in Amphioxus, the epibranchial groove. . . . . In the adult Amphioxus this organ is confined to the branchial region, but in the young it extends farther caudally. The great extension of the rudimentary organ above the gut of the higher forms is a secondary appearance.

There exists no ground for the view that the hypochorda arises from metameric dorsal entodermal diverticula. The fate of the epibranchial groove is connected with that of the hypobranchial groove, the modifications of the pharyngeal region causing both to become rudimentary. The hypochorda seems in general to degenerate, yet in part it is retained as an elastic band [see Franz, above]; perhaps its elements also play other roles. The reduction of the hypochorda is of significance of an important advance in the Chordate organism in that it renders possible the development of an unpaired [dorsal] aorta.

It is apparent that our most studied forms will repay new and careful study. A few years ago Dr. H. H. Wilder startled the world with his discovery that many of our salamanders were absolutely lungless, and now Professor F. Maurer, one of the most suggestive of German students, has made a strange discovery with regard to certain Amphibia. It is, in short, that in the oral epithelium of at least some terrestrial forms (Rana, Bufo, Hyla, Salamandra, *Triton alpestris*) capillary blood vessels pass through the basal membrane and penetrate the stratified epithelium. After a full description of the conditions found and a few remarks upon the histological and anatomical considerations, he concludes his paper (in the Morphol. Jahrbuch, Bd. xxv) with the suggestion that no doubt these vessels have an important physiological function in that they with the oral epithelium are concerned in respiration. Histological investigations of the oral epithelium of certain of our turtles where Gage (Am. Nat., xx, p. 233, 1886) has described a respiration in the mouth cavity might prove interesting.

Mr. W. G. Ridewood calls attention (Anat. Anzeiger, XIII, p. 499) to the fact that the cartilages recently described by Mr. White and Miss Foote as occurring in Elasmobranchs, are fully described in the older monographs on these forms.

The view advanced by Gervais and Lucus that Scolopendra is ovoviviparous has remained uncontradicted until the present time. Filippo Silvestri (Atti dei Lincei) now states that last year he discovered a specimen of *Scolopendra cingulata* carefully guarding its eggs under a stone, and in June of this year he has found several specimens with

their eggs. The ova are pale yellow, ellipsoidal in shape, measuring 2.5 x 3 mm.—*Nature*, August 26, 1897.

Zoological Articles in Recent Journals.—In Vol. 105 of the Sitzungsberichte of the Vienna Academy, Dr. Franz Werner writes upon the Scaling of the Reproduced Tail in Lizards; Dr. F. Steindacher upon some Zoological Results of the Expedition of the "Pola" to the northern part of the Red Sea; Dr. P. Knold upon the Blood Corpuscles of Vertebrates; and Dr. H. Albrecht upon the Comparative Anatomy of the Mammalian Larynx.

PSYCHOLOGY.[1]

Odor-mixture.—The relation of elementary sensations to the sensation of their compound has given rise to much theoretical discussion. In the senses of sight and hearing it has also been the subject of considerable experimental work. The laws of color-mixture have long since been formulated, and the sequence of the color series, like that of the tone series, is well known. In the domain of smell, owing to practical difficulties that attend the investigation, little progress has been made. Certain odors stand marked as qualitatively distinct, but their relations to one another and the arrangement of their "shades" into a single graduated series has never yet been satisfactorily demonstrated. On the other hand, it has been shown that odor-mixtures (of many odors, at least) give rise to new and qualitatively simple odors, thus resembling the color-mixtures rather than the accords of tone combinations. Zwaardemaker, in a recent work, gives a series of nine distinct classes of odors, into one or other of which he thinks any particular odor can be placed. He resolves compound odors into elements belonging to two or more of these classes. When the organ of smell is fatigued for one class of odors, the remaining elements in the compound are sensed, and if the compound consists of but two elements they may readily be distinguished by this means. Both this author and Aronsohn, an earlier writer, speak of certain odors which do not combine to form a mixture, but when placed together give rise to a *blended* sensation, each element of which may be sensed separatively at will. In some compounds, again, one element predominates so strongly that the other is wholly indistinguishable.

[1] Edited by Howard C. Warren, Princeton University, Princeton, N. J.

Nagel has lately taken up the investigation[2] by a different method—that of simply sensing the various compounds without fatiguing the organ of smell. As a result of his investigations he concludes that odor-mixtures without exception follow the law of color-mixture. When one element of a compound extinguishes the other it is because the former is of far greater intensity; but by reducing this intensity sufficiently a combination is at length reached in which the two unite to form a true mixture. He therefore takes exception to the earlier view, and believes that any two odors can be mixed in such proportions as to produce, at least momentarily, the sensation of a simple odor, of a quality distinct from the components. Whether the new odor is sensed as such permanently, or not, depends on the condition of the sense-organ; if the latter is less fatigued for some of the elements than for others, the former will gradually tend to predominate. The true color-mixture—that in which none of the elements predominate—"resembles each of its components, without, however, being like them." Thus the principles of odor-mixing, according to Dr. Nagel, are similar to those of color-mixing; and the correspondence extends, as far as the author's observation goes, to the law of intensity; the intensity of an odor-mixture is never stronger than that of its components. The author has found several pairs of odors that are more or less complementary and produce an almost odorless mixture, though he has never succeeded in reaching this limit. As regards the arrangement of simple odors into a series, Dr. Nagel's experiments do not tend to verify the classifications hitherto proposed; but he does not venture upon a classification of his own, since he has been unable to discover any odors which can be regarded as really "elementary."—H. C. W.

Psychology at the British Association.—At the Toronto meeting of the British Association, last August, a cordial invitation was given to psychologists to participate. There being no Psychological Section in the Association, the department was assigned to Section I (Physiology), and Dr. Kirschmann, of Toronto, was appointed a secretary of that Section to represent the interests of psychology in the arrangement of the program.

Among the papers of special psychological bearing presented in the Section were the following: On visual reaction to intermittent stimulation, by Dr. Grünbaum, of Cambridge, England; on the nature and physical basis of pain, by Prof. L. Witmer; on the physiology of instinct, by Prof. Lloyd Morgan; and two on various problems of

[2] Zeitsch. f. Psychologie, 1897, XV, p. 82.

animal psychology, by Prof. Wesley Mills. Physiological psychology was well represented on the program. Prof. H. P. Bowditch discussed the rhythmic action of smooth muscles, and Prof. Carl Huber, of Michigan, reported experiments on the innervation of motor tissues with special reference to nerve endings in the sensory muscle spindles; Prof. F. S. Lee, of Columbia, read a paper on the ear and lateral line in fishes, in which he discussed the bearing of the semi circular canals on the sense of equilibrium; and Prof. W. P. Lombard, of Michigan, discussed the effect of frequent excitations on the contractility of muscle. One session was devoted to the demonstration of physiological and psychological apparatus. Prof. Lombard exhibited a new and inexpensive type of chronograph. Prof. Scripture demonstrated the use of the Pendulum Chronoscope as a means for measuring small periods of time, and exhibited a "tricolor lantern" for illustrating the laws and effects of color combination. Prof. C. S. Sherrington performed some experiments in visual contrast and upon the flicker phenomenon; and Dr. J. H. Kellog exhibited a new dynamometer, especially adapted for clinical use. A combined meeting of the physiological and botanical Sections was held for the discussion of the chemistry and structure of the cell; Profs. Meldola, J. R. Green, and Macallum contributed papers at this session.

In addition to the foregoing, a number of papers having a distinctly psychological bearing were presented in Sections H (Anthropology) and D (Zoology). At one session of Section H several papers on Indian customs and folk-lore were contributed by Miss Fletcher and Messrs. Hill-Tout, R. N. Wilson and Hagar. At another session of the same Section reports were presented on the subject of anthropometric measurements in the schools, and the treatment of dull and slightly abnormal children. A paper by Dr. Franz Boas was read, embodying a statistical examination of the growth of Toronto school children; Prof. Witmer reported the results of some comparative tests—both mental and physical—between white men, white women and Indian men. The Presidental Address of Section H, by Sir. W. Turner, was an examination of some of the distinguishing characteristics of human structure.

In the zoological Section the subject of variation and selection received some attention. Prof. C. S. Minot spoke on the origin of vertebrata, and Prof. H. F. Osborn on the origin of mammalia. Prof. E. B. Poulton discussed the value of mimicry as evidence of the truth of Natural Selection; Mr. W. Garstang, of Plymouth, England, spoke on Recapitulation, and Prof. Lloyd Morgan on the natural history of

instinct. Two papers bearing on the genetic problem were also given in the anthropological Section; one by Mr. George Iles entitled: "Why Progress is in Leaps;" and a note by Prof. J. C. Ewart on the transmission of acquired characters.

Considerable material of interest and value to the psychologist was presented in these and other papers. The arrangement of the program, however, though admirable in most ways, was not especially suited to the exigencies of the department. Through the dividing up of these papers among three different Sections some were found to conflict with with each other; at best the auditor was compelled to watch his time closely, and literally *pursue* his subject from one building to another.—H. C. W.

SCIENTIFIC NEWS.

The Ninth Annual Meeting of the Association of Economic Entomologists was held at Detroit, Aug. 12–13, 1897.—The address of the retiring president, Prof. F. M. Webster, treated of "The Present and Future of Applied Economic Entomology in the United States," and contained, among other very interesting features, a tribute to the value of the systematist and a somewhat caustic criticism of the "species maker," helpful suggestions for the experiment station worker, and a very frank discussion of the unfortunate results which attend the attempts sometimes made to combine politics and science. Seven were elected to active membership and three foreign members were elected: The Association now consists of 93 active and 31 foreign members. Seventeen papers were presented during the sessions of the Association.

Resolutions were passed requesting the publication of the proceedings as bulletin of the Divsion of Entomology, U. S. Dept. of Agriculture and expressing familiarity with the efforts of the state of Massachusetts to exterminate the gypsy moth and commending the results already accomplished.

The election of officers resulted as follows: President, Herbert Osborn, Ames, Iowa; 1st Vice-President, Lawrence Bruner, Lincoln, Neb.; 2nd Vice-President, C. P. Gillette, Ft. Collins, Colo.; Secretary-Treasurer, C. L. Marlatt, Washington, D. C. The next meeting of the Association will be held at Boston, Mass., Aug. 19–20, 1898.

The Archæological Institute of America is about to commence the regular and uniform publication of its papers, reports, and other documents. For this purpose it has obtained control of the American Journal of Archæology, formerly edited by Professor Forthingham.

The new series of the Journal will be conducted by an editorial board chosen by the Council of the Institute, Professor John H. Wright, of Harvard, being the Editor-in-Chief. The publishers will be the Macmillan Company, New York.

The Academy of Sciences at Berlin has made the following grants in the aid of research:—Prof. Engler, 2000 mk. for African botany; Dr. R. Hesse, 500 mk. (eyes of lower marine animals); Prof. H. Hürthle, 850 mk. (study of muscles); Prof. Cohen, 1500 mk. (meteorites); Dr. G. Lindau, 900 mk. (Lichens) Prof. R. Bonnet, 800 mk. (blood vessels); Dr. Lühe, 2000 mk. (fauna of north African salt lakes); Prof. F. Frech, 1500 mk. (geology); Dr. G. Brandes, 300 mk. (Nemertines).

The total attendence of the German Universities for the summer semister of 1897 is reported as follows: Berlin, 5163; Munich, 4033; Leipzig, 3221; Bonn, 2015; Breslau, 1646: Halle, 1641; Freiburg 1544; Würzburg, 1443; Heidelberg, 1322; Tübingen, 1301; Göttingen, 1229; Erlangen, 1153; Marburg, 1097; Strasburg, 1047; Greifswald, 853; Kiel, 764; Jena, 754; Konigsburg, 737; Giessen, 692; Rostock, 509; Münster, 497.

We learn from the Botanical Gazette that Mr. William Wesley Woolen, a prominent citizen of Indianapolis and a member of the Indiana Academy of Sciences, has indicated his intention of presenting to the city a tract of fifty-six acres of land to be used as a botanical garden and ornithological preserve. The tract is easily accessible and is admirably adapted to these purposes. The details of management, etc. are now under consideration.

The U. S. Fish Commission is engaged in stocking the Penobscot river with the California quinnat salmon. Last year over 2,000,000 fry were turned loose in the river while in August of this year 35,000 young about two inches in length were put into the water, while 20,000 more are to be liberated soon. It will be some time before the results of these attempts can be ascertained, but even moderate success would fully repay all the outlay.

Henry Holt & Co. have recently published *The Elements of Comparative Zoology* by Prof. J. Sterling Kingsley of Tufts College. While containing the usual text-book information, it is more than usually full of laboratory illustration, and makes a special feature of suggestive questions under "Comparisons." The same house has issued, *Laboratory Directions in General Biology* by Harriet Randolph, instructor in Bryn Mawr College.

The Plant World, a new illustrated monthly botanical journal, is announced, to be published by Willard N. Clute & Co., Binghamton,

N. Y. at one dollar a year. It will be under the editorial charge of Dr. F. H. Knowlton, of Washington, assisted by six well-known botanists. It aims to fill a position intermediate between the technical and the amateur journals.

The Association of German Naturalists and Physicians met this year on September 20th to 25th at Brunswick. Among the more important papers read was one by Professor Waldeyer upon Impregnation and Heredity, the publication of which in full will be awaited with interest. The Deutsche Botanische Gesellschaft met at the same time and place.

The University of Minnesota has maintained a summer school this season for the sixth time with a good attendance, 14 took the work in entomology; 38 that in geology; 18 that in animal physiology and 12 that in vegetable physiology. Courses were also given in chemistry and physics, as well as several non-scientific subjects.

The annual report of the Essex Institute has just been issued. From it we learn that the annual income was $7,400, the membership 904 and the addition to the library, which now contains about 70,000 bound volumes and 200,000 pamphlets, amounted to over 9,000 entries.

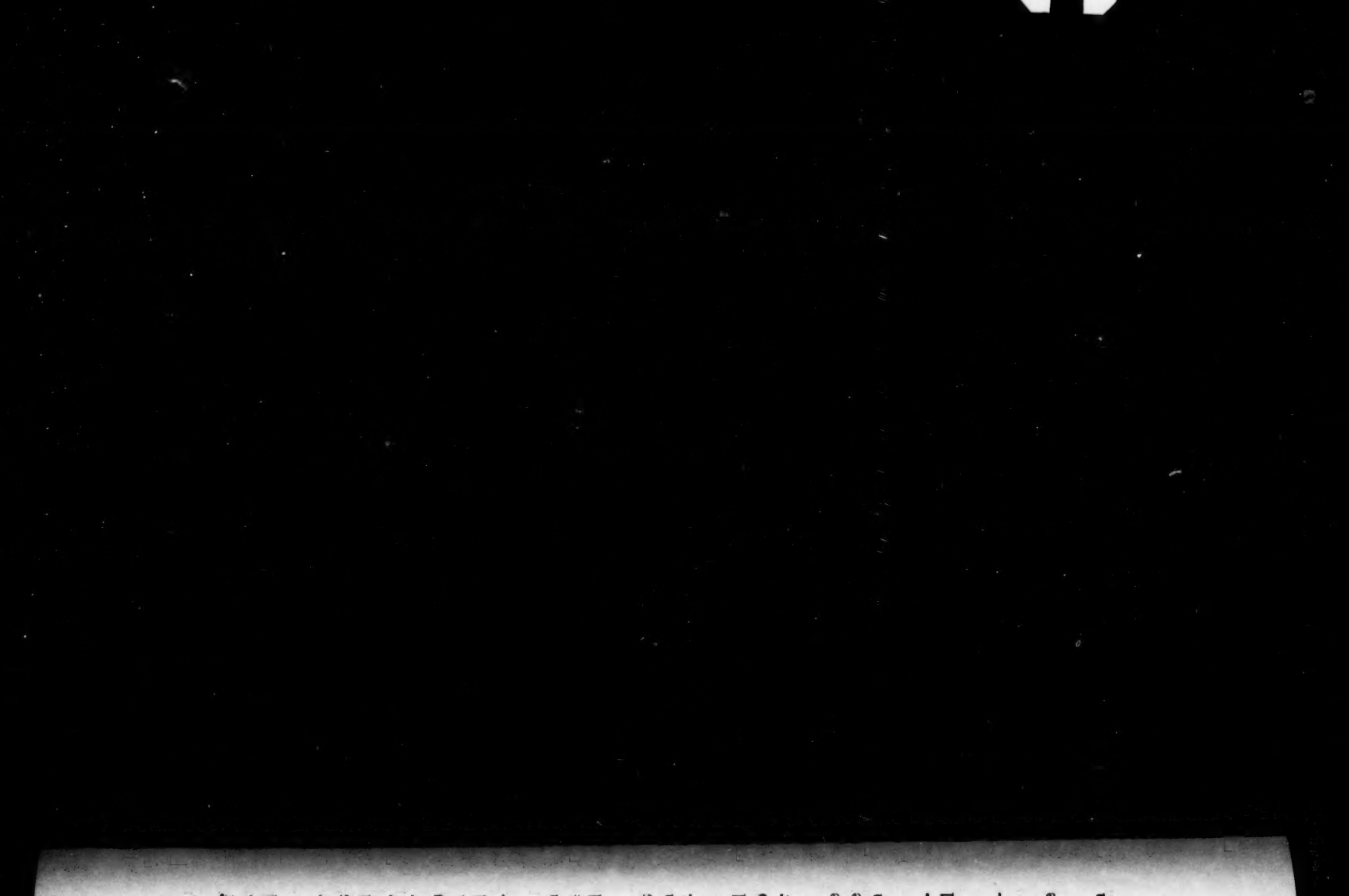
The American Museum of Natural History in New York has just received two large collections of butterflies. That of Mr. William Schaus contained over 10,000 tropical species; that of Dr. E. A. Hoffman is said to be complete in North American species.

Professor Albert von Kölliker has recently received the gold medal of the Leopold-Carolina Academy. We have previously referred to the memorials presented him upon the completion of his eighthieth year and the fifty year jubilee of his professorship.

The rules of the civil service of England have been set aside in the case of Dr. Henry Woodward who retains his position of keeper of the department of geology in the British Museum two years longer than the regulations otherwise would allow.

The University of Lyons has appropriated Fr. 42,000 for the completion of the biological laboratory at Tamaris, near Toulon, and will probably contribute annually to its maintenance.

We learn that there were hardly 400 in attendance upon the meeting of the French Academy of Science held this year at St. Etienne.

Dr. Bradley M. Davis of the University of Chicago will spend the coming year in Europe.

A slightly cracked egg of the great auk has just been sold in London for £168.